LIBRARY 18 DEC 2 1933

Vol. XVI, No. 6 *PSYCHOLOGICAL REVIEW PUBLICATIONS* DECEMBER, 1933

Journal of Experimental Psychology

EDITED BY
SAMUEL W. FERNBERGER, UNIVERSITY OF PENNSYLVANIA

HOWARD C. WARREN, PRINCETON UNIVERSITY (*Review*)
WALTER S. HUNTER, CLARK UNIVERSITY (*Index*)
HERBERT S. LANGFELD, PRINCETON UNIVERSITY (*Monographs*)
EDWARD S. ROBINSON, YALE UNIVERSITY (*Bulletin*)

CONTENTS

The Experience of Duration as Affected by Need-Tension: S. ROSENZWEIG AND A. G. KOHT 745

A Study of Organic Set: Immediate Reproduction of Spatial Patterns Presented by Successive Points to Different Senses: R. K. COMPTON AND P. T. YOUNG 775

Time-Errors in the Method of Constant Stimuli: C. C. PRATT..... 798

The Constancy of Gray with Constant and with Changing Illumination: C. O. WEBER 815

An Attempt to Condition Breathing and Eyelid Responses to a Subliminal Electric Stimulus: H. CASON AND N. KATCHER..... 831

The Influence of Certain Prior Conditions upon Learning: P. L. WHITELY AND G. BLANKFORT 843

Quantitative and Qualitative Essential Elements of Transfer: F. MCKINNEY. 854

The Experimental Control of Visual Factors: T. H. HOWELLS AND T. H. CUTLER 865

Discussion: The Inadequacy of the Concept of Unilateral Cerebral Dominance in Learning: S. I. FRANZ, 873. *A Note on Hartmann's Experiments Showing the Effect on Visual Acuity of Simultaneous Stimulation of Other Sense Organs:* C. H. HONZIK, 875.

PUBLISHED BI-MONTHLY
FOR THE AMERICAN PSYCHOLOGICAL ASSOCIATION
BY THE PSYCHOLOGICAL REVIEW COMPANY
PRINCE AND LEMON STS., LANCASTER, PA.
AND PRINCETON, N. J.

Entered as second-class matter, July 1, 1920, at the post-office at Lancaster, Pa.

PUBLICATIONS

OF THE

AMERICAN PSYCHOLOGICAL ASSOCIATION

EDITED BY

HOWARD C. WARREN, PRINCETON UNIVERSITY (*Review*)
S. W. FERNBERGER, UNIVERSITY OF PENNSYLVANIA (*J. Exper. Psych.*)
WALTER S. HUNTER, CLARK UNIVERSITY (*Index and Abstracts*)
HENRY T. MOORE, SKIDMORE COLLEGE (*J. Abn. and Soc. Psychol.*)
HERBERT S. LANGFELD, PRINCETON UNIVERSITY (*Monographs*)
EDWARD S. ROBINSON, YALE UNIVERSITY (*Bulletin*)

HERBERT S. LANGFELD, Business Editor

PSYCHOLOGICAL REVIEW

containing original contributions only, appears bi-monthly, January, March, May, July, September, and November, the six numbers comprising a volume of about 540 pages.

PSYCHOLOGICAL BULLETIN

containing critical reviews of books and articles, psychological news and notes, university notices, and announcements, appears monthly (10 numbers), the annual volume comprising about 720 pages. Special issues of the BULLETIN consist of general reviews of recent work in some department of psychology.

JOURNAL OF EXPERIMENTAL PSYCHOLOGY

containing original contributions of an experimental character, appears bi-monthly, February, April, June, August, October, and December, the six numbers comprising a volume of about 900 pages (from Jan. 1, 1933).

PSYCHOLOGICAL INDEX

is a compendious bibliography of books, monographs, and articles upon psychological and cognate topics that have appeared during the year. The INDEX is issued annually in June, and may be subscribed for in connection with the periodicals above, or purchased separately.

PSYCHOLOGICAL ABSTRACTS

appears monthly, the twelve numbers and an index supplement making a volume of about 700 pages. The journal is devoted to the publication of non-critical abstracts of the world's literature in psychology and closely related subjects.

PSYCHOLOGICAL MONOGRAPHS

consists of longer researches or treatises or collections of laboratory studies which it is important to publish promptly and as units. The price of single numbers varies according to their size. The MONOGRAPHS appear at irregular intervals and are gathered into volumes of about 500 pages.

JOURNAL OF ABNORMAL AND SOCIAL PSYCHOLOGY

appears quarterly, April, July, October, January, the four numbers comprising a volume of 448 pages. The journal contains original contributions in the field of abnormal and social psychology, reviews, notes and news.

ANNUAL SUBSCRIPTION RATES

Review: $5.50 (Foreign, $5.75). **Index:** $4.00 per volume.
Journal: $7.00 (Foreign, $7.25). **Monographs:** $6.00 per volume (Foreign, $6.30).
Bulletin: $6.00 (Foreign, $6.25). **Abstracts:** $6.00 (Foreign, $6.25).
Abnormal and Social: $5.00 (Foreign, $5.25). Single copies $1.50.
Current numbers: **Journal,** $1.25; **Review,** $1.00; **Abstracts,** 75c; **Bulletin,** 60c.

COMBINATION RATES

Review and Bulletin: $10.00 (Foreign, $10.50).
Review and J. Exp.: $11.00 (Foreign, $11.50).
Bulletin and J. Exp.: $12.00 (Foreign, $12.50).
Review, Bulletin, and J. Exp.: $16.00 (Foreign, $16.75).
Review, Bulletin, J. Exp., and Index: $19.00 (Foreign, $19.75).

Subscriptions, orders, and business communications should be sent to the

PSYCHOLOGICAL REVIEW COMPANY

PRINCETON, NEW JERSEY

Journal of
Experimental Psychology

VOL. XVI, No. 6 DECEMBER, 1933

THE EXPERIENCE OF DURATION AS AFFECTED BY NEED-TENSION

A PRELIMINARY STUDY

BY SAUL ROSENZWEIG
Harvard University
AND
AASE GRUDE KOHT
University of Oslo
From the Psychological Clinic, Harvard University

I. INTRODUCTION

In a series of experiments concerned with the relation of need-tension [1] to hedonic tone and memory, one of the present writers (S. R.) had the opportunity to notice that under conditions of emotional excitement and strain marked errors in the judgment of duration were sometimes made by the experimentees.[2] This was an incidental result, in no way provided for by the experimental procedure, but it was of sufficient significance to suggest the problem of the present investigation. That problem was as follows: What are the effects of need-tension upon the experience of temporal duration? or, in

[1] The meaning of 'need-tension' may be inferred from the discussion throughout the paper. It may, however, be well to explain at the very outset that all we mean by this term is a state of strain, accompanied sometimes by emotional excitement, that results whenever any need (equivalent on the conscious level to desire) is aroused. When a need is tending towards satisfaction or closure, need-tension may be assumed to exist.

[2] We shall speak of the Experimenter (Er) and of the Experimentee (Ee) in the present paper. The latter term is the exact equivalent of the German *Versuchsperson*.

other words: Is there any significant relation between the presence of a directional tension in the mind and the manner in which elapsed time is estimated?

One of the earliest problems to be worked on experimentally in Wundt's laboratory, when psychology was first becoming experimental, was that of temporal perception. This work was conceived in terms of a static, elementaristic system and was concerned with very brief time intervals such as a few seconds. Our work may be viewed as an attempt to treat the same problem, that of temporal perception, on a much more organismic or global level and from a dynamic systematic standpoint. To what extent such an approach is profitable it is not possible to predict but that the effort is worth while cannot be gainsaid. The investigation reported in this paper is avowedly of a preliminary character. We do not by any means believe that we have established any definite laws relative to our field of research, but we do believe that our results show the likelihood of a significant relation between the presence of need-tension and the experience of duration, and so set the stage for further investigation.

Münsterberg and Wylie (**8**) and Ebbinghaus (**4**) point out that we must distinguish the estimation of long time intervals, the judgment of which rests upon the manifoldness of presentations and is *indirect* throughout, from the estimation of lesser time lengths, in relation to which a direct time sense may be conceived. We have found it convenient to make a further division between the estimation of very short intervals, parts of a second or a minute, which seem to be *directly* observed as if by a pure time sense, and the estimation of intermediate time lengths, which are observed neither directly nor through much mediation of memory and other complex processes. Intermediate intervals might be said to range from about a minute to an hour. We should then have left very long intervals such as hours, days, months, and years, in the estimation of which memory enters as a very important factor. The limits of these interval classes are, of course, very difficult to draw and we do not wish to do so in any decided way, especially since the transitions are doubtless

gradual, and also because we know very little about the subject as yet. For heuristic purposes, however, such a classification is helpful.

What we know about these three subdivisions of the general problem of time estimation differs widely, just as the ways differ in which knowledge about them is acquired. The best experiments are done on the very short intervals, and laboratory knowledge is here greatest. Our knowledge about long periods is not great and comes chiefly from *belles-lettres*, such as *Der Zauberberg* by Thomas Mann.

In this paper we shall not be concerned with very long or very short time intervals. Our experiments involve intermediate lengths, ranging from one to ten minutes, the filling of which conforms closely to everyday experience. The experimental literature on this topic is not very extensive. The first experiments were performed by MacDougall (**7**) in 1904 and about a dozen researches have been since carried out. The results of this work are, moreover, very divergent. There is general agreement that the estimation of time differs with the character of the filling, *e.g.*, that time filled with action seems shorter than time spent in idleness or in some passive occupation such as listening to someone talking (**2, 5, 7, 9, 12, 13, 16, 19**). Time lengths of less than five minutes are found to be in general overestimated, but the experimenters do not fully agree. While Swift and McGeoch (**16**) and Spencer (**12**) maintain that all periods below five minutes are overestimated, others (**2, 7, 9, 13, 19**) are more conservative in their conclusions. Some writers (**2, 12, 19**) point out the favoring of numerical estimates ending in zero or five. Most of the writers (**2, 5, 7, 9, 16, 19**) have looked for sex differences, but the results are here again widely divergent and even contradictory. Whether women tend to overestimate more than men, whether they more generally give estimates ending in zero or five, whether they vary more than men—these seem to be as yet unsolved problems.

II. Technique

In nearly all cases, the method in the experiments which have been done upon intermediate times was such that the

Ees were asked *beforehand* to estimate a certain time interval which then ensued. An exception is an experiment of G. C. Myers (9) in which estimations were not asked for until after the period in question had elapsed. Our experiments resemble Myers' in this respect. They differ, however, in that estimates were requested by us *immediately* after the interval to be estimated had passed, not much later. Our experiments thus fall into an intermediate class in respect to the time at which estimates are requested, where an anticipatory attitude of estimation would not interfere with the development of the experience and where the memory of the experience would be still fresh and so not make estimation rather indirect.

Our Ees had the same sort of occupation during two periods of the same objective duration. The occupation was in each case a jig-saw puzzle of four pieces which looked rather easy to solve but which was in fact unsolvable. The difference between the two periods was brought about by the instructions which were so devised that one puzzle was attempted for practice whereas the other was performed as part of a test. Our intention was that there should be greater need-tension in the latter than in the former case.

When the Ee entered the experimental room and had been seated, the Er casually asked him if he had a watch that he wouldn't mind lending for use during the test, in view of the fact that the Er had broken his. If the Ee had a watch which he might later use to judge the elapsed time, this deprived him of it. There lay before the Ee on the table a completed four-piece puzzle, and pointing to this, the Er now asked: "Have you ever done puzzles of this sort?" and "Have you heard anything of the tests we do here?" The Er then went on to say that an intelligence test was soon to be given the Ee and that it involved such puzzles. The Er was, however, not fully ready to begin and would have to leave the Ee for a while to make certain preparations. It might be well for the Ee to do a puzzle simply for practice during the absence of the Er. With this, a puzzle similar to the completed one on the table, except for the fact that it was unsolvable, was produced and casually laid before the Ee. The Er now left the room and from an adjoining observation-chamber proceeded to observe and note down the behavior of the Ee. After a previously determined time, the Er returned, went directly to the puzzle on which the Ee had been working, removed the pieces and asked, "How long would you estimate that I was gone?" Then, without apparently taking much stock in the reply in order not to awaken any expectation of future estimations of time (the estimate was recorded immediately after the Ee had started to work on the next puzzle), the Er said, "I think we are ready to start the test now." The following instructions were then read:

"I am going to give you an intelligence test so that you may be compared for certain purposes with the other individuals who are taking the test. As I told you

before, the test will consist of some puzzles which you are to solve. The object is to put together the pieces of wood you are given each time to make a board eight inches square, like this one before you. You should work as rapidly as you can, for each puzzle is allotted a specified time within which it is to be solved; and if you do not finish in time, I shall naturally have to stop you. Is that clear?"

The Ee was now given a puzzle, again of four pieces and again an unsolvable one like the first puzzle. The timing of the activity was done by a stop-watch and was made obvious so as to arouse the Ee. Behavior notes were made. The amount of time allowed was always equal to that allowed for the practice puzzle. At the expiration of this period, the Er stopped the Ee with the word 'Time' and, immediately removing the pieces, asked, "How long would you estimate that you worked on this puzzle?" If the Ee gave exactly the same time as for the other puzzle, he was asked whether the one or the other period had seemed to him the longer. He was then given a sheet of blank paper and was read the following:

"Will you please write a paragraph in which you describe any feelings or emotions you have had during the time you spent in this room, and especially the feelings you had with respect to the passing of time, while you were working on the practice puzzle and while you were working on the test puzzle, comparing the two."

In the case of some of the Ees (*cf.* below) some supplementary questions were now asked. In concluding the experiment, the Ee was asked to coöperate by not discussing with anyone else any feature of the experiment in order that all individuals who might come would know only as much as he did before the test.

The Ees were 89 undergraduates of Harvard and Radcliffe Colleges. The experiments were grouped in two series as follows:

A. 52 Ees, 20 women and 32 men, tested by Miss Koht with intervals from one to ten minutes. Of these, 10 women and 11 men were from the psychology classes at the Clinic; the other 31 were Freshmen sent from the Students' Employment Office and remunerated for their services.

B. 1. 22 Ees, all women, 10 of whom were tested by Miss Koht, 12 by Mr. Rosenzweig, all with intervals of five minutes. All these Ees were Freshmen sent from the Students' Employment Office

2. 15 Ees, all men, tested by Mr. Rosenzweig. They were Freshmen sent from the Students' Employment Office. These Ees were asked some additional questions to be mentioned later. For 10 of them the general instructions were also slightly altered: instead of asking the Ee how long he would *estimate* the period to have been, he was asked how long the period *seemed* to have been.

III. Quantitative Results

The 89 Ees gave in all 176 estimates, one of the Ees being unable to make up his mind as to the required estimates. As compared with the times allowed in each case, 74 periods were overestimated, 33 were correctly estimated, and 69 were underestimated. Though so many of the estimates were correct, we cannot but feel that chance was an important factor here as the immediately following discussion will make clear.

One of the factors influencing estimation is, as Spencer (**12**) has pointed out, the preference for numbers with the end digit zero or five. Indeed, we got the estimate 'five minutes' in 49, or in about 28 percent, of all the cases. This is in rather good conformity with the results of Yerkes and Urban (**19**), who found the zero or five ending in a little more than one-third of their estimates, and with Axel (**2**), who found the five ending in 29 percent of his cases. The zero ending played a much less important rôle in our experiments because so many of the intervals were clearly less than ten minutes. It is of interest that in Series *B* the five minute estimate occurred in only 25 percent of the cases, or a little less frequently than when all the results are taken together. We cannot agree with Yerkes and Urban (**19**) that the tendency towards round number estimates is stronger in women than in men, for we find 25 of the 94 estimates by men to be five minutes (*i.e.*, 27 percent) and 23 of the 82 estimates by women to be five minutes (*i.e.*, 28 percent). It is true that we did not obtain as many estimates as Yerkes and Urban, but if there were a clear sex difference it ought to appear even here.

For our problem the tendency towards round number estimation is of no great importance, for we are not concerned with time estimation in relation to absolute physical times, but emphasize rather the relation between the two estimates given by the same Ee under different degrees of tension. It is, moreover, probable that the second period was seldom estimated independently, but always in comparison with the first. Having said 'five minutes' for the first period, the Ee, finding the second, say, a little shorter, is apt to give four or three minutes as his second estimate. The second estimate is probably relative to the standard created by the first estimate. The standards involved in the estimation may, however, be absolute in some way so that the relation of the estimates for the two periods is to be considered an indirect one which is achieved through reference to a common middle term as standard. To ascertain whether or not there would be any difference in the result arising from any formal factor of this sort, fifteen of the Ees (tested by S. R. in Series *B*) were asked

directly to compare the intervals as to length with each other, after they had given numerical estimates. This question came after the usual course of the experiment had been run. In practically every one of the cases the two methods of comparison yielded the same result for the relative length of the two periods.

Though we do not emphasize the objective correctness of the estimates, we found it of interest to analyze a little more carefully the question of overestimation and underestimation. As we have already said, we got the same number of overestimations as of underestimations. But it seemed to be the case that short intervals were more often overestimated than long ones. To ascertain how far this was so, we divided our estimates into three groups as follows: (1) The estimates where the actual times were from one to three minutes, (2) where they were from four to seven minutes, (3) where they were from eight to ten minutes. Table I shows the results of this analysis.

TABLE I

OBJECTIVE LENGTH OF THE INTERVAL IN RELATION TO OVERESTIMATION AND UNDERESTIMATION

	Length of Period in Minutes	Overestimations	Correct Estimations	Underestimations	Total
Series *A*	1–3	19 (63 percent)	5 (17 percent)	6 (20 percent)	30
	4–7	23 (55 percent)	9 (21 percent)	10 (24 percent)	42
	8–10	15 (47 percent)	1 (3 percent)	16 (50 percent)	32
Series *B*	5	17 (24 percent)	18 (25 percent)	37 (51 percent)	72

Overestimations tend to diminish as the objective time interval increases and underestimations tend to increase as the objective time interval increases. The great number of correct estimations in the 4–7 minute period and in the 5 minute series is obviously due to the round number tendency. The great number of underestimations in the 5 minute series is surprising if one compares the previous literature on the subject. It is possible that the conditions of tension present in our experiment, and probably less present in previous ones, is a determining factor in this connection. With respect to

sex differences in overestimation and underestimation, we found nothing significant.

We have up to now been discussing some fairly general results of our experiments, incidental to their main purpose. In now reporting our more important results, all the cases in Series *A* may be considered together for there are no important differences between the parts of this series. The sex of the Ees made no difference great enough to warrant separate treatment, nor did membership in the psychology class. The psychological knowledge of the Ees from the class might be expected to give them a different attitude towards the experiment, but this seems not to have been the case. We find, in the verbal reports, that some of the psychologically trained Ees used technical terms and sometimes a more technical style, but the experience and feelings behind the words do not seem notably different from those expressed by the more usual words of the Freshmen. We may also consider the results in Series *B* as unitary. The five minute interval was selected on the basis of an analysis of our results in Series *A* from which it appeared (*cf.* below) that periods longer or shorter than this were less adapted to produce the desired conditions. For some of the Ees the instructions were a little modified. Mr. Rosenzweig asked ten of his male Freshmen how long the time *seemed* to be instead of asking how long they would *estimate* it to be. The reason for so doing was to ascertain whether the attempt to 'estimate' the times introduced any extraneous factors instead of yielding an unintellectualized direct report of the previous experience. This change had, as far as we can see, no influence at all on the answers. We therefore see no reason for not treating the results in this group as a unit.

As has already been pointed out, our chief purpose was to compare the two estimates given by each Ee, irrespective of their relation to the objective time length or the absolute difference between them. We should then have three possible results: the first estimate might be larger, equal to, or less than the second. When equal estimates were made, the Er always asked for a differentiation; but it sometimes happened

that the Ee insisted on the equality of the two periods. Table II summarizes the results under this heading. It indicates a

TABLE II

FREQUENCY OF CASES IN WHICH THE FIRST OR SECOND PERIOD WAS JUDGED THE LONGER

	Objective Time in Minutes	Number of Ees Estimating the First Period as Longer	Number of Ees Estimating the Two Periods as Equal	Number of Ees Estimating the First Period as Shorter
Series *A*	1–10	29	4	19
Series *B*	5	22	0	14

tendency for the practice period to be estimated as the longer.[3]

The numerical amount of the difference between the estimates for the two periods is naturally not the same for the different time allowances. It ranges from being scarcely noticeable, when the Ee gives the same number for both periods but admits that one of them seemed to be the longer, up to a difference of five minutes. Table III gives these re-

TABLE III

OBJECTIVE LENGTH OF THE INTERVAL IN RELATION TO THE DIFFERENCE BETWEEN THE ESTIMATED LENGTH OF THE TWO PERIODS

	Objective Time in Minutes	First Estimate the Larger			First Estimate the Smaller		
		Average Difference	Maximum Difference	Minimum Difference	Average Difference	Maximum Difference	Minimum Difference
Series *A*	1–3	0.88	1.5	0.5	1.42	4.0	0.17
	4–7	1.61	5.0	1.0	2.54	5.0	1.0
	8–10	2.36	5.0	1.0	1.88	5.0	2.0
Series *B*	5	1.26	3.0	0.25	1.64	5.0	0.5

[3] Though we do not believe that much importance can be attached to the exact number that the Ees gave in their estimates, nor that the relation between the estimates and the actual objective time is of much significance, we attempted to discover whether there was any marked difference between the relation which the estimates of the first period bore to the objective time as compared with the relation of the second period to the objective time. We could find no significant difference here apart from what would naturally, though not necessarily, be expected from the already mentioned fact that the first period is in general said to be longer than the second. There is a slight tendency for the first period to be more often overestimated (in relation to the objective time) than the second. We do not desire, however, to create a false impression by dwelling upon this point.

sults. All the data in this table are in terms of minutes. We have counted differences between estimates that come out only upon interrogation as zero in the calculation to determine the averages of the differences, whereas the equal cases, where no differentiation is felt to be possible, are not counted at all. Minimum difference means the least difference *mentioned in numbers*, differences derived from questioning after equal estimates being excluded. We do not attach any significance to the exact number of these averages; we mention them only to give some idea of the order of size of the differences.

We attempted to find out whether there was any correlation between the length of the objective time and the relation between the estimates. Table IV gives the results. It ap-

TABLE IV

OBJECTIVE LENGTH OF THE INTERVAL IN RELATION TO RELATIVE LENGTH OF THE ESTIMATES

	Objective Time in Minutes	Number of Cases in Which the First Estimate is the Larger	Number of Cases in Which the Estimates are Equal	Number of Cases in Which the First Estimate is the Smaller
Series *A*	1–3	8	3	4
	4–7	14	0	7
	8–10	7	1	8
Series *B*	5	22	0	14

pears from this that the time lengths around five minutes are more apt to produce a relationship between the two estimates such that the first is greater than the second. In general, however, whether the physical time is long or short seems to have no marked influence on the relation between the paired estimates.

One of the reasons why we both did part of the experiments in Series *B* was to determine whether the sex of the Er had any influence on the results. This influence cannot be very important as Table V indicates. It is true that one girl became so nervous during her test by Mr. Rosenzweig that it was impossible to get any clear estimates. But, on the whole, we find no clear-cut differences in this respect. The dif-

TABLE V

THE EFFECT OF THE SEX OF THE EXPERIMENTER UPON THE RELATIVE SIZE OF THE ESTIMATES

	Er and Ee of Same Sex			Er and Ee of Opposite Sex		
	Women Miss Koht	Men Mr. Rosenzweig	Total	Men Miss Koht	Women Mr. Rosenzweig	Total
1st period longer	6	8	14	5	8	13
1st period shorter	4	7	11	4	3	7

ferences between the groups may rather be attributed to sex differences of and in the Ees.

With respect now to the sex differences of the Ees we have the results shown in Table VI. It is clear that the women

TABLE VI

SEX DIFFERENCES OF THE EXPERIMENTEES IN RELATION TO RELATIVE SIZE OF THE ESTIMATES

	Women			Men		
	Series *A*	Series *B*	Total	Series *A*	Series *B*	Total
First estimate the larger	14	14	28	15	8	23
Both periods equal	1	0	1	3	0	3
First estimate the smaller	5	7	12	14	7	21

showed the tendency of estimating the second period as shorter than the first much more frequently than did the men. The significance of this we shall discuss in the next section.

In brief the main quantitative result of the experiments is that there was a tendency for the Ees to estimate the first or practice period as longer than the second or test period. Fifty-one Ees showed this tendency; 33 the opposite; 4 estimated the periods as equal; and 1 was unable to give any definite estimations. The typical result was given more frequently by the women than by the men.

IV. QUALITATIVE RESULTS

The behavior of the Ees was closely watched during both the periods in all the experiments. This, taken in conjunction with the verbal reports written by the Ees immediately after

the experiment, gives us a rather clear idea of the general attitude of the Ees towards the experiment as well as any differences in attitude during the two periods.

The typical attitudes seem to be three in number: (1) The Ee may feel bored, filled with ennui; (2) he may be interested in his task, eager to finish it and hopeful as to his own ability to solve the problem; or (3) he may be in despair, feel that he cannot succeed in the task, that it is hopeless and so wish to get rid of the problem and away from the disturbing situation.

When the Ee experiences ennui he works lazily with the pieces of the puzzle, looks away absent-mindedly, yawns, studies the charts on the walls of the room, etc. In the verbal report he says he "wondered when you [the Er] would come back," that "each minute seemed about five," etc.

When the Ee is eager, interested and hopeful, he works quietly and rather quickly. He keeps his eyes fixed on the puzzle, moves the pieces more or less systematically, frowns and gives other external expressions of thinking. He may murmur to himself or sing or whistle, in order to help his own thinking or to give outlet to the tension which the interest creates. In his verbal report the Ee then says that "every second was spent thinking of ways in which to piece the puzzle together"; that "I became so interested that I did not notice the passage of time." He often feels the pressing of time, 'urged to hurry,' 'a trifle nervous because of the limit of time,' etc.

When the Ee is in despair he works in a way much like that during ennui. He moves the pieces slowly and aimlessly. He seems not to be concentrating and may let his eyes wander. But his facial expression is different; he looks hopeless, sad, worried. He looks toward the stop-watch or at the Er. Often he smiles in an apologetic way or so as to evoke sympathy. Sometimes he talks or asks questions. On the whole, he uses every means to get away from the actual situation, to have it stopped or to get into some mental by-path where the problem does not concern him. In the verbal report he then says that he felt 'ill at ease, and restless and disgusted,' "rather disgusted that I cannot do them" or "felt as though I

were wasting time on a useless thing." R. B. Perry (**10**) gives an excellent characterization of this attitude in the following:

> "There is a limiting point at which baffled interest passes over into despair and apathy. At this point the interest must decline or disappear because there is no aspect of the situation which suggests its consummation. The end must cease to motivate because there is nothing which presents itself as means. There is no interest-judgment which carries conviction, the only objects of response being irrelevant, useless or insuperable. As 'hope deferred maketh the heart sick,' so hope extinguished deadens the heart altogether" (pp. 556–57).

Of course we find intermediate conditions and transitions from one attitude to another. An Ee may start in one period with interest, gradually lose it and so grow bored; or he may start with good hope of accomplishing the task and then give up hope and grow disgusted. Interest may also increase; one may start with the thought that this is easy to do, but, finding that one is not able to solve the problem at once, one grows more interested and more occupied with the work. Or interest may be decreased by the discovery or belief that the puzzle is unsolvable; one then grows bored from being obliged to remain at it.

The attitude of the Ee is typically different during the two periods of the experiment. Typically in the first or practice period there is ennui or eagerness. Despair does not often occur. When the practice puzzle is taken as a means for 'killing time' until the Er comes back, ennui is usually felt unless the puzzle itself elicits interest. Often the Ee feels this interest from the first moment, interest in the task or curiosity as to his own ability to solve the problem. Sometimes he takes the practice puzzle as a test. He will then hurry so as to have it finished when the experimenter comes back, for he suspects that he is to be ranked as to success or failure on this puzzle.

During the test period we find all of the above mentioned attitudes. The Ee may be bored, especially if he believes the puzzle to be unsolvable.[4] More frequently he is anxious to

[4] The suspicion that a puzzle is unsolvable may arise even if the puzzle is solvable. We quote from the instructions of 'The famous Amos 'a' Andy Checker Board Puzzle': "If you should fail to solve this puzzle or are skeptical that it cannot be put together, sent 10¢ in coin or stamps and we will mail you correct solution." Such suspicion may arise as a self-defense when a puzzle has tricky difficulties, and can therefore never be completely avoided.

solve the puzzle in the given time. This attitude may last until the period ends. But it may also change into boredom, if the puzzle comes to be suspected of having no solution, or into despair, if the Ee loses all hope of being able to succeed. In the latter case he then grows worried, disgusted, bothered; he has a "feeling of disgust and almost despair as I could find no possible solution of the problem," and he may 'constantly wish to be stopped.'

The attitude to some extent depends upon the length of the period. When the period is short, one to three minutes, the Ee is less apt, because of the shortness of the time, to discover that the puzzle is probably unsolvable; he will not have sufficient time to be really bored or feel deep despair. When the time is of intermediate length, four to seven minutes, he may feel all three things, the tendency to give up the work being, however, rather rare and the probability of judging the puzzle to be unsolvable not very great.[5] When the time is long, from eight to ten minutes, the Ee may feel extremely bored during both periods; it is difficult to keep his interest for such a long while. He may be disgusted by both puzzles or bored by both. He may prove to himself that the puzzle is impossible. He may begin to think of other things, and the emotional spirit of these new thoughts may greatly influence his estimation of the time.

The attitude depends largely also on the individual himself, his predispositions and his previous experiences. Those who approach the test knowing of themselves that "I am not clever in solving puzzles," naturally assume a different attitude from that assumed by those who think that "I have always done fairly well in intelligence tests." Further, the need for superiority may be of different strength. The desire to do well in this *particular* test may differ. A Freshman who comes only to earn money will not care if he does well or not and will probably have a feeling of ennui during both periods; but a man from the class who is not paid, who comes out of psychological interest and may think that the result of this 'intelligence test' is to be reported to his pro-

[5] These are the reasons that influenced our choice of five minutes as the time length for Series *B*.

fessor, may desire intensely to do well during the test period. Some of the Ees from the classes actually asked if "anybody in the class had managed to do the puzzle," or if "the subjects usually find it out."

Failure on the first puzzle may influence the attitude towards the second. Some may feel lost from the beginning of the test because of their previous failure; some may feel more familiar with the test puzzle because of their previous experience. The presence of another person may have differing effects. Some Ees feel unhappy when alone; they are bored and wish another person to stay with them. Others feel at ease when alone, but quite annoyed if anyone is observing them when they are failing to solve the puzzle; they want 'not to seem stupid.' Others again do not especially care; they are absorbed in the problem and seem unaffected by the presence or absence of the Er.

Now, *there seems to be a correlation between these attitudes of the Ees during the two periods and the relative length of the time estimates. When the Ee is bored or feels despair time seems long; when he is interested and eager, time seems short.* The development of the attitudes during the Ee's activity seems to be rather important. A decrease in interest tends to make time seem longer, while an increase in interest makes it seem shorter. The final impression of the period seems of greater importance than the first in determining the estimate.[6]

The sex differences mentioned in the preceding section may now be considered. It will be recalled that the girls more frequently showed the general tendency we have found towards estimating the first period as the longer. The reason for this may lie in the differing *attitudes of the men and women.* The girls seem to assume the rôle suggested to them more readily than the men do; they seem in our experiments to be more submissive, to enter into the immediate situation with fewer reservations, suspicions or preconceptions. The men seem to be much more suspicious; they appear to have a

[6] To test the value of the hypothesis about the relation between estimation and attitude, one of the Ers (A.G.K.) tried to guess the relative size of the estimates before the Ee had given the second estimate. This was done in her seven last cases. Of these 7 guesses, 5 were correct, 2 incorrect.

stronger need for superiority and do their utmost to solve the puzzle, if it is possible, or to prove that it is unsolvable if they fail to solve it. They often resist the suggestions of the test instructions and create their own situation, regardless of that intended for them. This difference would naturally bring it about that the women would manifest the tendency to estimate the second (test) period as the shorter much more frequently than the men.

To illustrate the qualitative results and their relationship to the quantitative results, some examples from the two series of experiments are appended here.

First let us take some examples of behavior and verbal reports from the cases in which the practice period is estimated as the longer.

Case 1 (woman)
Time allowed—5 minutes
1st estimate—6 minutes
2d estimate—5 minutes

Behavior (Abstracted Notes)

Works quietly and incessantly during practice; rapidly and at random during test; at last looks exasperated.

Verbal Report

"While working with the practice puzzle, I enjoyed myself, it was a game, time did not seem to be shorter, but there was more of a spirit of fun. But working with the test puzzle I was conscious of strain, feelings of gee! I must be stupid not to be able to do this. I noticed the experimenter writing and wondered if she were writing about the silly things I was doing to the puzzle. It was much less enjoyable than the practice puzzle."

The great strain during the test is here the chief difference between the two periods. This is what we have termed interest, and it seems to be this factor which causes the second period to seem shorter than the first.

Case 2 (woman)
Time allowed—5 minutes
1st estimate—4 minutes
2d estimate—3 minutes

Behavior

Seems to think while working slowly with the practice puzzle; continues even after entrance of the experimenter. During test, works seriously, very systematically and carefully; looks sad when stopped.

Verbal Report

"When I worked on the practice puzzle I felt no hurriedness or need to think. I did not think of the passing of time.

"When I knew I had started the test I felt more alert and tried to keep my reason on the puzzle, the location of right angles and rough sides, and forget the sense of having to hurry.

"I felt a slight disappointment when I heard 'Time'."

Case 3 (woman)
Time allowed—5 minutes
1st estimate—4 or 5 minutes
2d estimate—3 minutes

Behavior

Works rapidly but a little unsteadily during practice, rapidly and nervously during test period.

Verbal Report

"When I first came, naturally this being my first experience of the sort I was filled with curiosity—I believe curiosity and anxiety to know what was going to happen next were probably the only emotions I had. While I was working with the practice puzzle the feeling of curiosity continued and I was becoming anxious for time to pass so that the man may return and the test to proceed. I was told that that was merely the practice one and therefore wasn't very much concerned at not getting it. I thought I might be able to get the others, so in that respect I wasn't very much pressed for time. While doing the other puzzle which I was told was part of the test, I was anxious for time to drag out so that perhaps I could eventually puzzle it out. Towards the end I became quite disgusted because I couldn't put them together and I believe I was quite relieved when the time was over because I felt then that time was no longer a factor in accomplishing the task."

In the practice period interest in the task is followed by ennui; in the test period despair comes toward the end, eagerness being, however, the most prominent attitude.

The following is a case in which the numerical estimates for the two periods are equal but in which, upon questioning, the first period is declared the longer.

Case 4 (man)
Time allowed—5 minutes
1st estimate—6 minutes
2d estimate—6 minutes (less)

Behavior

During practice period, quiet and serious; smiles and shakes his head, later pouts. During test: quiet at first; talks, saying that he doesn't like it and that it is 'far beyond me.'

Verbal Report

"While working on the test puzzle I felt that I was going to fail to work the problem and that I should fail to get a reasonable grade in the test. I did not think that the pieces would fit. I was worried as to the length of time it would take me to finish, and I felt greatly relieved when the time was up.

"While I was working on the real test I had a mental picture of failure. I was more excited than in the practice problem for this time my result was being recorded. I was very disgusted at my failure to solve the problem and constantly wished that I would be stopped."

In the first period the Ee is bored; he also feels inferior. In the second, he is disgusted and wishes to escape. The combination of ennui and disgust in the first period and the even stronger disgust in the second make them both of about the same length.

We present now two examples of cases in which the test period is estimated as the longer.

Case 5 (woman)
Time allowed—5 minutes
1st estimate—5 minutes
2d estimate—7 minutes

Behavior

Works carefully during practice period, smiling now and then. Works eagerly during test; looks up and smiles once.

Verbal Report

"While working on the practice puzzle, I felt as though what seemed such a simple thing should be easy to solve. The time went very quickly, but I was glad, because I did not like to work on it at all and I seemed to get nowhere. While working on the second puzzle, I really tried to get it to go together. The time seemed to pass slowly, however, and I felt that it seemed hopeless and useless to try to put the puzzle together. I felt as though I were wasting time on a useless thing, and wondered how such things could entertain some people."

The Ee is bored during both periods. Ennui is mingled with disgust: "I did not like to do it at all." Hopelessness is more pronounced during the test; the feeling of futility makes the second period seem longer.

Case 6 (man)
Time allowed—7 minutes
1st estimate—5 minutes
2d estimate—10 minutes

Behavior

Works quietly during practice period, perhaps a little impatient. More rapidly during test; seems nervous; a little confused; more nervous as time passes.

Verbal Report

"While working on the practice puzzle, after a few minutes had elapsed, the four pieces seemed to be quite the wrong shape. I had tried every conceivable position and had found only two of the pieces to fit. At the start, when I had hope of putting the pieces together the time seemed rather quick in passing. As I could get no results, however, a feeling of disgust took me and time seemed very slow. During the experiment the time seemed to pass more quickly, although I felt that I had worked longer. The sound of the experimenter's pencil helped to make time more real. I had however the same feeling of disgust and almost despair as I could find no possible solution of the problem."

In this case we have disgust in both periods, but it is stronger in the test period than during the practice. The test period seemed then to be the longer.

The general results of this qualitative analysis of the experiments are as follows: When ennui or despair is the most prominent feeling, especially when ennui or despair is increasing during a period, time tends to be estimated as longer than when eager interest and hope predominate.

V. Theoretical Considerations

Before attempting to set forth the theoretical considerations that seem to us to shed light upon these experimental results, we must call attention to certain obvious shortcomings in these experiments. In further work these defects should be offset. We mention them here so that no misleading impression of conclusiveness may be carried away from our experimental data or from the theoretical remarks of this section. The shortcomings are as follows:

1. The fact that the puzzles we employed were unsolvable created suspicion to a degree probably not to be expected from solvable puzzles. The extent to which this interfered with the experiment it is impossible to estimate, but we have the impression that our results would have been *more striking* in the absence of this difficulty.

2. The same puzzle was always given in the same period. This is technically a fault. We do not, however, believe that it made much, if any, difference in our results because of the close similarity of the two puzzles to each other.

3. We have a constant difference between the two periods of our experiment in the respect that the Ee was alone in the room during the first period but never so during the second. We believe, however, that this is one factor in the complex of factors that produced the difference in the amount of need-tension present during the two periods. The Ee was alone during the period when it was desired that need-tension be slight, observed during the period when a rise in need-tension was desired. We therefore feel that this difference directly contributed to the establishment of the very conditions we were seeking to establish.

4. There is a constant difference in the order of the practice and the test periods that should have been cancelled out. This seemed so important to us that we tested twenty-one additional Ees reversing the order of the test and practice performance of the puzzle. It is obvious that difficulties entered with this variation. One cannot well ask an Ee to practice *after* the test has begun. For this reason the Er made some excuse for leaving the room after the first test puzzle, offering the Ee a puzzle to do for his amusement, *i.e.*, 'not as a part of the test.' But then the difficulty was present that the unclosed need of the immediately preceding failure would persist during this second performance. This second period would then not be strictly comparable to the first (practice) period of the previous series. In spite of such inherent handicaps our results indicate that the test period was even here more frequently judged to be the shorter of the two periods. Of the 21 Ees, all of whom were tested with five

minute intervals, 10 estimated the first or *test* period to be the shorter, 4 estimated the two periods to be of equal length, 6 estimated the second period to be the longer, and 1 result is uncertain. The results were not completely univocal, but they were much better than could have been expected in view of the difficulties involved in trying to change the order without concomitantly changing the degree of tension in the two periods.

5. We are not perfectly satisfied that the difference between having the Ee 'estimate' the periods and having him say low long they 'seemed' has been fully investigated. Our results on this head are too few to give an adequate answer to this problem and our technique on this point was not adequate, it seems to us.

6. The difference between indirect (numerical) and direct comparison of the lengths of the two periods is still open to question though our results seem to make this difference of no importance in these experiments.

All of these factors must be investigated further and until this is done our present results are sheerly tentative. But supposing for argument's sake that they all were shown to make no essential difference to the experimental results we have obtained, how should we then be able to explain these results? It is in such a spirit that the following suggestions are offered. We can obviously not be satisfied with the comprehensive speculations of the philosophers, nor can we rely upon precise physiological reductions. We shall therefore be content with purely psychological considerations. These are, incidentally, considerations that do not necessarily stand or fall with the experimental data we have obtained. They might have been profitably offered as a synthesis of or supplement to the various previous theories on temporal experience simply on the strength of common sense observations combined with logical analysis.

1. Attention on Time vs. Attention on Task

It has been frequently stated that when attention is focussed on a task, time seems comparatively short, while if attention is paid to the passing of time itself, duration is

subjectively lengthened. The direction of attention is thus made out to be a decisive factor in durative experience (**6**).

In our experiments this explanation seems to be applicable in only a special sense. During the test period the attention of the Ee was naturally much more concentrated upon the task than in the practice period. This should tend to decrease the subjective duration of the test period and in general this is our result. But it was also during this same period that interest in the passing of time was very great. The Ee knew he was being timed; the stop-watch was within view constantly and the fatality of being stopped after a limited time hung over him. This was not, however, the case in the practice period. The result of this should then be a subjective lengthening of the test time. This is, however, the very opposite of the first conclusion relative to the influence of the direction of attention on the subjective length of the test period. How is the apparent contradiction to be resolved?

Perhaps it is to be resolved by reference to the *attitude* with which one contemplates the passing of time. Whenever it is said that attention to the passing of time increases its subjective length, there is an implication such as is contained in the proverb "A watched pot never boils." What is meant is that when we attend to time *with the desire for it to be short* it seems to be long. This was presumably not the attitude with which our Ees in the test period paid attention to the time; it is more than likely to have been the very opposite because of the then existing need for the time to be as long as possible. If this is so, however, the fact of attending to time—or to the task in hand—becomes less important than the need-attitude accompanying the particular direction of attention.

2. Empty vs. Filled Time

The importance of filling in time estimation has often been pointed out (**6**). The usual formulation is that empty time seems longer than filled time. The problem, just considered, of the subjective lengthening of an interval that comes about from paying attention to time's passing introduces us to the consideration of this factor. For it is usually the emptiness of time that leads us to pay attention to the duration itself.

In our experiments this explanation is applicable again only in a special sense. Ostensibly both periods of the experiment were filled in the same way—puzzle-solving. We should therefore expect that this factor would not affect the results. But, on the other hand, it often happened that the Ee did not work as continuously on the practice as on the test puzzle. He sometimes stopped to look around the room, etc. The practice period would, as a result, be less completely filled than the test period in which no time was to be wasted if a good showing were to be made. This difference in filling might then have influenced the judgment of duration so that the practice period—being less filled—would be judged the longer.

But it is to be noted that whatever difference in filling between the two periods existed was functionally related to the need-attitudes of the Ee during the two periods. Moreover, the effect of difference in filling upon time estimation is in general probably a function or correlate of need-attitude. James (**6**) describes an 'unsatisfied desire for change' that persists during empty time and continues: "The *odiousness* of the whole experience comes from its insipidity: for *stimulation* is the indispensable requisite for pleasure in an experience, and the feeling of bare time is the least stimulating experience we can have. The sensation of taedium is a *protest*, say Volkmann, against the entire present" (p. 626). Such statements as these do seem to offer some explanation of the experience of duration, but terms such as 'protest' and 'unsatisfied desire for change' introduce dynamic concepts that we must examine more closely.

3. Pleasantness vs. Unpleasantness

Time pleasantly filled has been said to seem shorter than time unpleasantly filled (**14**). As Lamartine writes, "Je dis a cette nuit sois plus lente, et l'aurore vient dissiper la nuit."

In our experiment the period generally regarded as the shorter—the test period—should then have been the more pleasant. In order to check this hypothesis, fifteen of the Ees were asked at the end of the session and without in any way interfering with the regular course of the experiment, "Which of the two experiences (the first or the second period) was the

more pleasant?" Of the 15 Ees thus questioned, 13 designated the practice period as more pleasant, only 2 the test period. To probe more deeply, of the 13 who had regarded the practice period as more pleasant, 6 had estimated it as longer, 7 as shorter than the test period. The two who called the test more pleasant had estimated the practice period to be the longer. It is certainly not possible to conclude from this that the relative pleasantness of the experience by itself produced the general tendency to regard the practice period as the longer.

When asked why they had preferred one period to the other, the Ees generally said that the second period, because it was a test, had oppressed them more, made them feel stupid and pressed for time; whereas in the first period they had had little if any reason for hurrying or worrying or for feeling embarrassed. On the whole it was the existence of an articulate need that made the second puzzle less pleasant than the first. Without the presence of an articulate need the eventuality of failure or of success does not enter to influence affection to any great degree. One of the Ees made a remark, however, that leads us to a significant insight: "If I had solved either puzzle, that would have been the more pleasant. Since I solved neither, the practice puzzle was the more pleasant." It is deducible from this that solution of the problem would have been the truly pleasant aspect of either experience. In the absence of this eventuality, the practice puzzle is the more pleasant because it is the puzzle in which failure was less keenly felt. In other words, it is not that the practice puzzle is really pleasant but that it is less unpleasant than the test puzzle. A corollary to this, however, is the following: If both puzzles had been solved the test puzzle would have given more pleasure, and it is probably in connection with this that the need-tension incident to the solving of the puzzle is aroused.

This point is extremely important if we attempt to peer behind the pleasantness or unpleasantness attendant upon the experiences. It means more to the Ee to solve the second puzzle than it does to solve the first. More is at stake; more is

anticipated; it would be more pleasant to win out in this case than in that of the practice puzzle. We shall later see that it is these factors lying back of the affective judgments of the Ees that probably determine the relative size of the time estimates. The pleasantness or unpleasantness of the experiences are themselves related to need-attitudes.

4. Desire for Time to be Long vs. Desire for Time to be Short

In a note published in 1903 Binet (**3**) reports a case of erroneous temporal judgment in a woman suffering from insomnia. He writes that for this patient "la nuit d'insomnie est courte, parce qu'elle la voudrait longue" (p. 21)—in order that she may get the necessary quota of sleep. On the other hand, after a sleepless night, "elle désire donc que la journée soit courte pour que la nuit réparatrice arrive vite; désirant que la journée d'attente soit courte, elle la trouve longue" (p. 21). Binet concludes his brief account by stating that he considered it worth while reporting this case because "c'est un exemple qui montre nettement que certains phénomènes de perception, qui semblent entièrement composés de processus élémentaires, sont en réalité sous la dépendance de fonctions psychiques très complexes" (p. 21).

We have already described the part apparently played by the work-attitude of our Ees in the experience of elapsed time, and in the immediately preceding theoretical discussion the importance of attitude has repeatedly forced itself upon our attention. Is it possible that Binet's observation has relations with our experiment?

If we hypothetically translate our earlier description of the attitudes adopted by our Ees into terms of desiring the time to be long or short, we have the following: In general the Ees desire the time of the practice period to be shorter and of the test to be longer. They desire the practice period to be shorter because it is tedious for them after a certain time. They desire the test period to be longer in order that they may have a greater chance of solving the puzzle. The result—in keeping with Binet—is that the practice period is generally regarded as longer than the test period.[7]

[7] We are not saying that in relation to the objective time the practice period is overestimated and the test period underestimated. We are here only comparing the estimates of the two periods with each other, not with the objective time length.

To check this generalization the same fifteen Ees whom we interrogated on the question of pleasantness and unpleasantness were asked: "Did you during either period—the practice or the test—want the time to pass more slowly or more quickly than it seemed to be passing?" [8] The answers given by these Ees can certainly not be conclusive proof or disproof of anything, but something better than speculation, though nothing as good as pure observation, is here involved.

The 15 Ees answered as follows:

A. Of the 8 in the group who judged the practice period to be the longer,

Two said they had wanted the time to go slower in the test period and faster in the practice period;
Two were conscious merely of having wanted time to go slower in the test period;
Three had no report to make in this connection;
One had wanted the time to pass faster than it was passing during the test period.

Thus far then the hypothesis is corroborated by a proportion of about four to one.[9]

B. Of the 7 Ees who estimated the test period to be the longer,

Three signified that they had wanted the time to go faster than it was going during the test period;
One said he had felt there was 'enough time' in both periods;
One had wanted the time to go slower in both periods;
One had wanted the time to go slower in the test period;

[8] It would take us too far afield to discuss in detail the relation of the subjective *length* to the subjective *rate* of time. We have up to now spoken of *short* or *long* time; now we refer to time passing *slowly* or *quickly*. In general the statement that time passed quickly is equivalent to the one that it was short, and correspondingly with slow and long. In accordance with Binet's notion, we should then have the following: Time goes slowly when we want it to go quickly and quickly when we want it to go slowly.

[9] This summary statement omits from consideration those just previously mentioned cases in which no report was offered, since here any want, if it existed, did not reach consciousness. A want or desire may, however, influence the judgment of duration without rising into consciousness, just as the fact of such influence itself is surely not conscious even when the wish is.

One said that no such desire entered his mind, but "he supposed it would be natural" to want the time to go more slowly during the test period.

This last report in view of its deductive character cannot be taken as indicative of anything psychologically experienced; it is avowedly an opinion, not an introspection. Of the 6 remaining cases, 3 are completely in conformity with the theory, 1 is more so than not, while 2 are wholly negative. Again the tendency is in the right direction.

On the basis of these introspections, if we omit as we must the no-report and the ambiguous cases, we have seven cases in which the time estimates are in conformity with the Binet formula against three where this is not so. This is considerably better than what we had when, not relating estimates to desire, we considered all cases in which the test period was judged to be longer than the practice as irrevocably negative.

How shall we account for these facts? Thus far we have arrived at the point of seeing that, as Binet observed, time seems to be short and go quickly when we want it to be long and go slowly, and vice versa. But this formula is scarcely an explanation: the facts seem to bear it out but, except in so far as it introduces dynamic factors, it still leaves us in the dark as to the explanatory principle behind the descriptive paradox.

Sully (**15**) has made a suggestion that goes deeper than the descriptive formula of Binet. Considering the hypothesis that attention to the passing of time lengthens its subjective duration, he writes: "This principle must be supplemented by another consideration. Suppose that I am very desirous that time should *not* pass quickly. If, for example, I am enjoying myself or indulging in idleness, and know that I have to be off to keep a not very agreeable engagement in a quarter of an hour, time will seem to pass too rapidly; and this not because my thoughts are diverted from the fact of its transition, for on the contrary, they are reverting to it more than they usually do, but because my wish to lengthen the interval leads me to represent the unwelcome moment as further off than it actually is, in other words, to construct an ideal representation of the period in contrast with which the real duration looks miserably short" (p. 251).

The contrast between the projected ideal or desired state and the actual state of affairs is a very plausible explanation of the facts we have been considering. Another, which may be regarded as a variation of this one, invokes the principle of direct projection and its corollary, wishful thinking. Consider the case of a young child who has been playing in the street for a rather long time with some companions. His mother calls him into the house to do some disagreeable task. What does he answer? "I've been playing only a minute!" On the other hand, let the same child be occupied with the disagreeable task and let one of his companions call him out to play. He asks his mother if he may go and, if she reminds him that his task is unfinished, he will probably retort, "I've been working all day!" It is not of great importance whether the child is actually making an estimate of time in these cases. The likelihood still remains that to some extent he really feels what he says about the time in spite of the exaggeration he permits himself. He has played 'only a minute' because he wishes he had only started to play; wishing he could play for a long total time, he would like it to be the case that he had been playing only for a short time, more thus remaining; time then seems to have been short and to have passed very quickly. The judgment of time is a projection of a wish. In the case of the time of working, the attitude is the reverse: wishing that the total time should pass quickly and be short, one projects the wish into the judgment and the time that has already elapsed is said to be long, thus leaving less still to lapse.

In general then and within certain limits, it is as if we put as much time behind us and leave as much ahead of us as we wish there to be. We say that much time has passed when we would like this to be the case; little time, when we would like this to be the case. The desire—conscious or unconscious—or the attitude toward time's passing to some extent distorts our judgment of duration. Wishful thinking enters here as in so many everyday experiences.[10]

[10] This interpretation, as well as our results, accords with a recent suggestion of Adams (1) on the problem of learning. He maintains that the fundamental task of the psychology of learning is to determine the laws in accordance with which the obstruction

It is worth noting that we have introduced the function of judgment in this discussion. It is necessary to do so in view of the manner in which the time estimates of which we are speaking were made. It would be erroneous to suppose that we have sheer perception in these cases. More complex mental processes are here involved and this must be borne in mind if the purport of these considerations is to be understood.

We may summarize this theoretical discussion as follows: Comparatively speaking, duration is subjectively long when we want it to be short and short when we want it to be long. When we want time to be short, we are often very attentive to its passing as is especially apparent in case time is empty or unpleasantly filled. When, on the other hand, we want time to be long, we are ordinarily attending to what is occupying us, and in this case time is probably filled and, moreover, pleasantly or with an anticipation of pleasure. It should be clear from this that need-attitude may be regarded either as one factor among others that operates independently of them in the judgment of duration, or as a factor in some way functionally related to various others invoked in previous discussions of temporal experience. In either case the importance of this dynamic factor is clear. The principle through which the need-attitude operates in order to influence subjective duration is unknown but may be one of projection with contrast, or of direct projection. Such in general are the interpretative notions suggested by our experiments. The extent of their validity is to be determined by further investigation.

Summary

In some experiments upon the estimation of time intervals ranging from one to ten minutes in length but filled so as to produce different degrees of need-tension, a general tendency was found for periods in which there is relatively greater need-tension to be estimated as shorter than periods in which there

of a need distorts the 'field' in which it occurs. He adds: "At present we know practically nothing about it. I have a little evidence that, under the influence of a very strong and completely obstructed need, the field contracts spatially. I should be inclined to guess that, under certain conditions, it would also contract temporally, but that again can only be settled by experiment" (p. 164).

is less. Eighty-nine Ees participated but for various reasons the results cannot be considered to be more than tentative. Certain theoretical suggestions are offered in which attention is directed to the similarity of the phenomena under consideration to certain everyday instances of wishful thinking.

References

(1) Adams, D. K., A restatement of the problem of learning, *Brit. J. Psychol.* (Gen. Sec.), 1931, **22**, 150–178.
(2) Axel, R., Estimation of time, *Arch. Psychol.*, 1924, **12**, 1–77.
(3) Binet, A., Note sur l'appréciation du temps, *Arch. de Psychol.*, 1903, **2**, 20–21.
(4) Ebbinghaus, H., Grundzüge der Psychologie, I, Dritte Auflage, Leipzig, 1911, 496–510.
(5) Gulliksen, H., The influence of occupation upon the perception of time, *J. Exper. Psychol.*, 1927, **10**, 52–59.
(6) James, W., Principles of Psychology, I, New York, 1890, 605–642.
(7) MacDougall, R., Sex differences in the sense of time, *Science*, N. S., 1904, **19**, 707–708.
(8) Münsterberg, H., and Wylie, A. R. T., Optical time-content, *Psychol. Rev.*, 1894, **1**, 51–56.
(9) Myers, G. C., Incidental perception, *J. Exper. Psychol.*, 1916, **1**, 339–350.
(10) Perry, R. B., General Theory of Value, New York, 1926, 556–557.
(11) Quandt, J., Das Problem des Zeitbewusstseins (Literaturbericht), *Arch. f. d. ges Psychol.*, 1906, **8**, 143–189.
(12) Spencer, L. T., An experiment in time estimation using different interpolations. *Amer. J. Psychol.*, 1921, **32**, 557–562.
(13) Sturt, M., Experiments on the estimation of duration, *Brit. J. Psychol.* (Gen. Sec.), 1923, **13**, 382–388.
(14) Sturt, M., The Psychology of Time, New York, 1925, pp. 158.
(15) Sully, J., Illusions: A Psychological Study, New York, 1891, 245–261; 302–305.
(16) Swift, E. Y., and McGeoch, J. A., An experimental study of the perception of filled and empty time, *J. Exper. Psychol.*, 1925, **8**, 240–249.
(17) Titchener, E. B., A Textbook of Psychology, New York, 1910, 340–348.
(18) Volkmann, W., Lehrbuch der Psychologie von Standpunkt des Realismus, II, Cöthen, 1885, 11–36.
(19) Yerkes, R. M., and Urban, F. M., Time-estimation in its relation to sex, age, and physiological rhythms, *Harvard Psychol. Stud.*, 1906, **2**, 405–430.

(Manuscript received August 11, 1932)

A STUDY OF ORGANIC SET: IMMEDIATE REPRODUCTION OF SPATIAL PATTERNS PRESENTED BY SUCCESSIVE POINTS TO DIFFERENT SENSES

BY RICHARD K. COMPTON AND PAUL THOMAS YOUNG

University of Illinois

The term 'set' is used by psychologists to cover various kinds of predisposition, attitude, posture, preparatory adjustment, and the like. In one form or another the notion of setting the organism comes into every field of psychological research. Reference to set (*Einstellung*) occurs in the early experimental literature upon lifted weights, in the literature of reaction, and of association.[1] In the sphere of psychophysics [2] the subject's attitude in relation to his psychophysical judgment is a matter of considerable importance. In the experimental studies of thought [3] the determining tendency (*determinierende Tendenz*), the problem (*Aufgabe*), and the 'cortical set' play dominant rôles. Experiments upon learning generally stress the importance of attitude—the 'will to learn.' Indeed, as noted in an earlier study,[4] the concept of set in one form or another comes to light in almost every field of experimental psychology.[5] To trace out all the ramifications of this concept is a well nigh hopeless task.

[1] An historical statement and appropriate references are given by May who also suggests a convenient classification of the meanings of set. M. A. May, The mechanism of controlled association, *Arch. Psychol.*, **5**, 1917 (No. 39). See also, A. T. Jersild, Mental set and shift, *Arch. Psychol.*, **14**, 1927 (No. 89).

[2] See the following papers and references given: S. W. Fernberger, Instructions and the psychophysical limens, *Amer. J. Psychol.*, 1931, **43**, 361–376; E. G. Boring, The control of attitude in psychophysical experiments, *Psychol. Rev.*, 1920, **27**, 440–452; S. S. George, Attitude in relation to the psychophysical judgment, *Amer. J. Psychol.*, 1917, **28**, 1–37.

[3] E. B. Titchener, Experimental Psychology of the Thought Processes, Macmillan, N. Y., 1909.

[4] P. T. Young, The phenomena of organic set, *Psychol. Rev.*, 1925, **32**, 472–8.

[5] Bills and Brown have shown that the efficiency of work and the relative decrements in work curves depend upon the amount of work that S is set to accomplish. A. G. Bills and C. Brown, The quantitative set, *J. Exper. Psychol.*, 1929, **12**, 301–323.

Development of Problem.—The present investigation is a direct outgrowth and continuation of experiments dealing with organic set in the human subject.[6] In the former experiments S was seated before a board containing ten electric lamps arranged in a circle. After a warning signal a number (3, 4, 5, or 6) of lamps flashed one at a time in irregular sequence. S was instructed to observe the lights and after the last had flashed to point in order to the places where the lights had appeared. If he pointed correctly, this was taken as an indication that a temporary organic set had been established which served as the immediate determiner of his pointing reaction.

We regard set as a specific and definite neural (or neuromuscular) adjustment established by stimulation and existing in the body as a determiner of the pattern of reaction.[7] Although our experiments do not shed light directly upon the detailed cerebral mechanisms involved in getting set, they do so indirectly by showing how the establishment of a set varies with conditions. The age of the S, practice, the speed of presenting stimulations, the number of stimulations in a presentation, the kind of figure made by the stimulus-points, and other conditions determine the complexity of set which can be established in an individual. In the experiment described below these variables are held constant or balanced out and one other factor is controlled: the sensory mode of presentation.

If assuming a temporary set is mainly a cerebral process, it should not matter very much whether S's task is presented to the eye, the ear, or the organs of touch. If, on the other hand, sensory processes play a considerable rôle in getting set, distinct differences in performance dependent upon the mode of presentation should appear. Just what relation exists between the central neural factors and the peripheral sensory

[6] R. Gundlach, D. A. Rothschild and P. T. Young, A test and analysis of 'set,' *J. Exper. Psychol.*, 1927, 10, 247–280.

[7] In the case of true memory it is necessary to assume a more or less permanent neural set. Professor Warren uses the term *set* to describe the relatively permanent trace left in nerve substance by nerve impulses. H. C. Warren, Elements of Human Psychology, Houghton Mifflin, N. Y., 1922, 45, 331.

factors in establishing a temporary set? In other words, how far is the establishment of a temporary set dependent upon the sensory mode of presentation and how far upon neural (or neuromuscular) processes?

Plan of Experiment.—To answer this question spatial patterns were presented by successive points in three sense departments—vision, audition, touch—and S was instructed to reproduce the patterns by pressing appropriate signal keys. Every pattern had six points. Sometimes all points were visual, sometimes all auditory, sometimes all tactual, sometimes mixed. Seven kinds of presentation were used: visual, auditory, tactual, visual-auditory, visual-tactual, auditory-tactual, visual-auditory-tactual. The same pattern-sequences were presented in all of these modes.

Originally we planned to use patterns of five points and built the apparatus with five visual, five auditory, and five tactual stimulus-positions. Later we decided to use six-point patterns and repeated one of the five positions twice in a pattern.

The stimuli were simple lights, sounds or touches of equal duration. The sensory experiences were of average intensity and of approximately equal intensity in the three sense departments. Intensities near the limen and intensities so great as to induce unpleasantness were avoided. Every stimulus had a definite place in S's spatial field and the stimuli were separated far enough to insure that errors of spatial discrimination and localization could not easily exist.[8]

Practice was equalized for the different modes of presentation and for the patterns presented.

The general aim was to reveal how far the establishing of a relatively complex set is dependent upon the sensory mode of presentation and how far it is dependent upon processes common to all modes.

[8] In preliminary tests we made sure that the stimuli used could be localized without error. Also at the close of the main experiment a test of absolute localization was given. Each of the 50 Ss received a series of 30 presentations—15 stimuli given twice in chance order. Conditions of presentation were similar to those of the main experiment. The performance of all Ss was perfect except for one error of one S in touch position No. 5 (see description of apparatus).

Conditions of Experiment

Apparatus.—The apparatus consists of three essential parts: (1) The *presentation apparatus* (Plate I) for stimulating S with discrete lights, sounds, and touches. Light stimulation is given by miniature mazda lamps (2.5 volt) located slightly below the level of S's eyes. These lamps are shown in Plate I at A, B, C, D, E. Sound stimulation is derived from the 60-cycle vibration of a telephone diaphragm. Five telephone receivers are located at *Q*, *R*, *S*, *T*, *U* behind cards which obscure them from S's view. The hum from each receiver is adjusted in loudness and character to be like that from all the others. Tactual stimulation is given by sharp metal points which rise up silently through holes in the table at positions *I*, *J*, *K*, *L*, *M* making contact with the flexion surface of S's forearms and left palm. The tactual stimulators have electro-magnetic action; one of the action-devices is shown at the right of Plate II, indicated by letter *Z*. It is a modified piano action provided with armature and arranged so that the stimulus-point remains raised in position as long as the circuit is closed through the electro-magnet.

The table holding the presentation apparatus is fastened to the floor at one end of the experimental room. S's chair is placed directly in front of the table in a central position; it is held in place by cleats on the floor. S rests his forearms on a felt cushion which is cut out so as to accommodate the arms and guarantee that tactual stimulators make proper contact.

S can easily reach five signal keys 1, 2, 3, 4, 5 (Plate I). Silent action of the signal keys is obtained by means of a felt-mounted contact point which makes contact when pressed into a mercury well.

The apparatus is so planned that the five tactual stimulus-points are on radii 30° apart. Lamps and telephone receivers are farther removed from S on the same five radii. Signal keys are just below the lamps and receivers, also on the common radii.[9]

(2) The *control apparatus*, located across the room from the presentation apparatus (Plate II). At the left of the table is a timer which consists of a 4 R.P.M. synchronous motor with a rotating arm and contact brush. The brush rotates over a copper disk cut in segments to give a series of contacts each 0.7 sec. in duration with approximately 0 interval between them. Six contacts of the timer are connected in order with six telephone plugs of the control panel (which are shown in Plate II suspended beneath the numbers 1, 2, 3, 4, 5, 6). By placing the plugs in the holes of the control panel the experimenter can determine which stimulus will come first, which second, which third, and so on. All six stimulations may be lights, all sounds, all touches, or they may be mixed in any desired combination.

The timer has a special contact for sounding a warning buzzer exactly 2 sec. before the first contact of the series. The buzzer sounds for 0.7 sec.

The signal keys 1, 2, 3, 4, 5 (Plate I) are used by S in reproducing the pattern-sequence. If the reproduction is correct, the six signal lamps (shown directly above 1, 2, 3, 4, 5, 6 in Plate II) flash in order except for the lamp which corresponds to the repeated stimulus.[10] The subject's response was recorded on the spot. Plate II shows the position of the record blank and pencil used in recording S's performances.

(3) The *electrical connections* between presentation and control apparatus. When

---

[9] Preliminary work showed that both sounds and touches could be localized with 100 percent precision when the stimulators were separated by 30°. See footnote 8.

[10] The repeated stimulus is signaled by a second flash of its lamp, *e.g.*, the sequence 545321 is signaled as 1, 2, 1, 4, 5, 6.

a telephone plug is inserted into one of the double-contact telephone jacks two electrical circuits are closed simultaneously. One of these circuits is used for stimulating S and the other for connecting S's signal key with the appropriate signal lamp of the control panel. To be specific: the sleeve contacts of the telephone jacks *A, B, C, D, E* (Plate II) are connected respectively with the stimulus-lamps *A, B, C, D, E* (Plate I) and this circuit carries electrical impulses from the timer to the stimulus-lamps. The tip contacts of jacks *A, B, C, D, E* are connected respectively with the signal keys 1, 2, 3, 4, 5 (Plate I); and through the telephone plugs these tip contacts are also connected with the signal lamps of the control panel 1, 2, 3, 4, 5 (Plate II).

The sleeve and tip contacts of jacks *F, G, H* are in parallel connection with those of *A, C, E* respectively so that any one of these three lights can be given twice in a presentation.

The same system of connections is used between the middle row of jacks and touch stimulators, and between the bottom row of jacks and auditory stimulators. The connections are so arranged that one set of signal keys 1, 2, 3, 4, 5 (Plate I) is used to indicate the position of lights, sounds and touches.

The wires connecting the presentation and control apparatus are bound into a cable.[11]

A direct current of 6 volts is supplied by a storage battery (under table, Plate II). The full current is used to activate the tactual stimulators. A resistance is introduced in the circuit for stimulus- and signal-lamps. A 60-cycle current of 7.5 volts produces a hum in the telephone receivers. This current is obtained by reducing the 110-volt University current; it is thrown into circuit by relays which are controlled by the 6-volt direct current. The relays are in an adjoining room of the laboratory.

The experimenter has two master keys (left of Plate II). One of these controls the timer; the other controls the battery current which makes the presentation.

Procedure.—S was seated comfortably in front of the presentation apparatus. Cleats on the floor held the chair at the center front of the table and at a constant distance from it. The experimenter demonstrated the placement of the arms over the tactual stimulators. The flexion surface of the left arm and the left palm covered tactual stimulators *I, J, K* (Plate I) and the right arm similarly covered *L* and *M*, the right hand resting lightly on the left. S was then given the following typewritten instructions:

> The apparatus in front of you will present a pattern of six lights, sound, or touches. Sometimes all six will be lights, sometimes all will be sounds, sometimes all will be touches, sometimes they will be mixed.
>
> A buzzer will give two seconds warning before the presentation.
>
> Attend closely to the presentations as they come, especially to the *position* and *order*.
>
> Count out loud as they come, saying—1, 2, 3, 4, 5, 6.[12]

---

[11] A detailed account of the wiring with two blue-print diagrams may be found in a doctoral thesis on file at the University of Illinois library. Details are omitted here but the plan of electrical connections should be clear from the discussion of the control apparatus. Richard Kenneth Compton, An Experimental and Analytical Study of Organic Set, 1932 (doctoral thesis, University of Illinois).

[12] Previous experience has shown that if the Ss are left to their own devices, some of them assign numbers or letters to the stimulus-positions and retain a verbal surrogate. Counting eliminates the naming reaction and the likelihood of purely verbal retention.

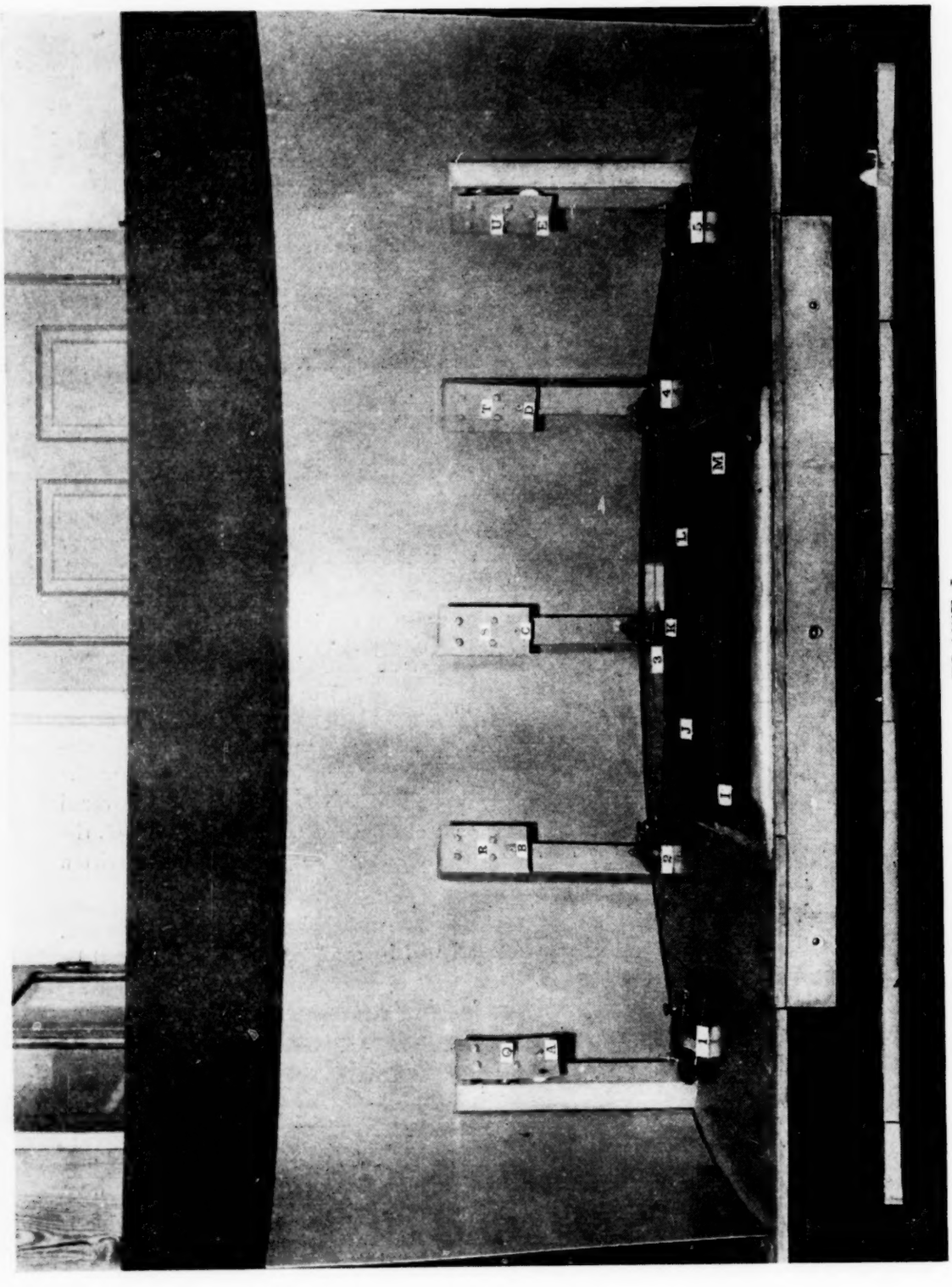

PLATE I
PRESENTATION APPARATUS

PLATE I
PRESENTATION APPARATUS

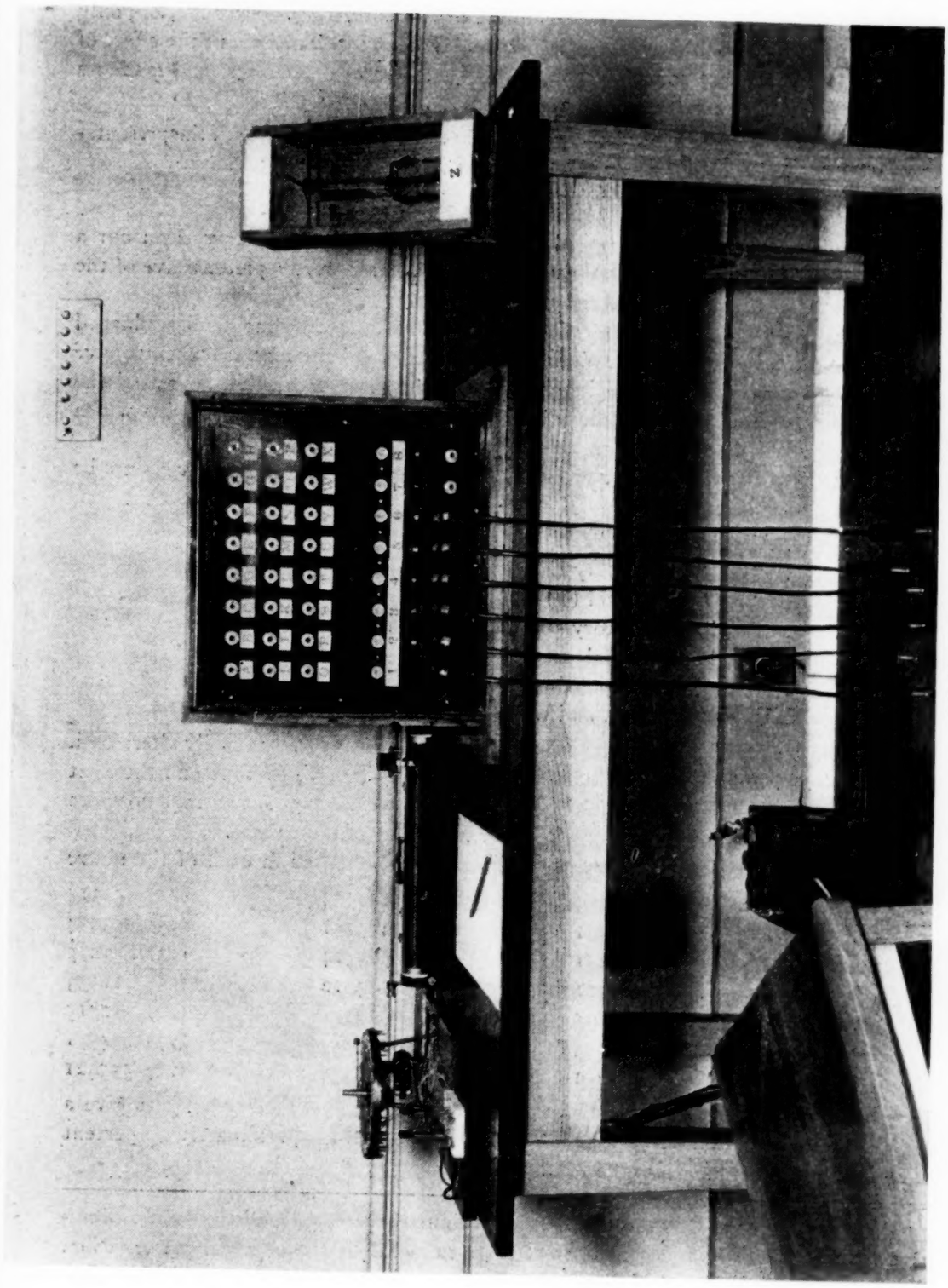

PLATE II
CONTROL APPARATUS

Although the Ss were not instructed to count during the reproduction of a pattern most of them did so. In a questionary at the close of the experiment 27 Ss said that they counted silently and 17 that they counted out loud during the reproduction; only 4 replied that they didn't count; 2 didn't reply. When questioned about the effect of counting 34 replied that counting was helpful, 7 said it was automatic and made no difference, 8 said it was a hindrance, 1 didn't reply.

You are instructed to reproduce immediately the *position* and *order* of the presentations by pressing the signal keys. Use the right hand in pressing the keys.

Do as well as you can and don't worry if you miss.

To make sure that S understood the instructions and could carry them out a practice series of four patterns was given. These patterns were representative of the different types of presentation described below.

In a single presentation the procedure was as follows: the experimenter first plugged into the control panel the pattern desired. He then closed the master switch to give the warning signal and the stimulus-pattern. After the presentation S pressed the signal keys reproducing the stimulus-pattern. Record was made of the lights which flashed on the control panel, which indicated omissions and errors.

The Patterns Presented.—The seven modes of presentation will for convenience be designated by letter as follows:

1. All stimuli visual (V)
2. " " auditory (A)
3. " " tactual (T)
4. Visual and auditory stimuli alternating (VA)
5. Visual and tactual " " (VT)
6. Auditory and tactual " " (AT)
7. Visual, auditory, and tactual stimuli in rotation (VAT).

In the mixed modes of presentation the type of stimulus was systematically alternated. For example, with the VA mode the first pattern was V, A, V, A, V, A and the next pattern was A, V, A, V, A, V. This plan was followed throughout so that no sense department had any advantage of priority or frequency of occurrence.

Twenty-eight patterns[13] were selected and were presented in each of the above seven modes. The patterns were chosen on the basis of preliminary work to give a wide range of difficulty. We wanted the patterns to yield approximately 50 percent of correct reproductions from the total group.[14]

(1) 352431	(8) 525134	(15) 154325	(22) 452315
(2) 345123	(9) 251341	(16) 123541	(23) 213534
(3) 545321	(10) 143251	(17) 323514	(24) 451235
(4) 321345	(11) 514321	(18) 154312	(25) 412353
(5) 253134	(12) 341235	(19) 512341	(26) 432515
(6) 524135	(13) 131245	(20) 121354	(27) 234531
(7) 245135	(14) 432151	(21) 231354	(28) 535421

[13] The patterns are given below. The five numerals correspond to the five stimulus-positions (Plate I) and the order of these numerals is the order of presentation. Position 1, 3, or 5 is repeated in each pattern.

[14] Practice gave a transition from 30.2 percent correct for the total group on the first day to 58.1 percent on the last. The total percentage of correct reactions for the experiment as a whole was 44.8 percent.

Each pattern was presented once in each mode, making a total of 196 presentations (28 × 7) per S. The total series of presentations was planned so as to distribute practice equally among the 7 modes. In the first 7 trials each mode was used once, in the second 7 each mode was used once, and so on. The 28 patterns were distributed so as to equalize practice among them and thus to guarantee that differences found depend wholly upon the mode of presentation.

The total series of 196 presentations was broken into 4 equal groups.[15] Each S had 4 sittings with 49 presentations at a sitting. The experimental periods were exactly one week apart.

Subjects.—Fifty Ss volunteered their services. Each came once a week for four consecutive weeks always at the same hour of day. The Ss were college sophomores, juniors and seniors. There were 33 men and 17 women—regarded as a single group because sex differences cannot be found in the results.

Scoring of Results.—At the close of the experiment the mimeographed data sheets contained a record of patterns presented and responses made by S. Every pattern was rated as right or wrong, no attempt being made to evaluate errors of reproduction. Comparisons in the following statement of results are based upon the number of correct reproductions.

Results

Differences Dependent upon Mode of Presentation.—Table I shows the average number of correct reproductions by the 50 Ss for each mode of presentation. The table gives also the

TABLE I

Average Number of Correct Reproductions with Different Modes of Presentation

Mode of Presentation	Average No. of Correct Reproductions out of 28 Presentations	σ_{dis}	P.E.$_M$
Aud.	16.22	5.14	.49
Vis.	15.86	5.46	.52
Vis.-aud.	14.96	5.15	.49
Tact.	10.70	4.36	.42
Aud.-tact.	10.42	5.52	.53
Vis.-aud.-tact.	10.40	5.67	.54
Vis.-tact.	9.24	5.55	.53

standard deviation of the distribution and the probable error of the mean for each mode. Table I should be read as follows: with auditory presentation the 50 Ss averaged 16.22 correct reproductions out of the 28 patterns presented; the standard deviation for the distribution of averages is 5.14; the probable error of the mean is .49.

[15] All of the patterns were expressed in terms of a common letter code as an aid to the experimenter. Letters are shown on the control panel of Plate II.

The averages of Table I are ranked in order of magnitude from highest to lowest. In some cases the differences between the averages are small and not significant.[16] The V mode stands only 0.36 points below the A (critical ratio 1.58). The difference between V and VA modes is small and of doubtful significance (critical ratio 3.35). The T, AT, and VAT modes are exceedingly close together and the differences are not significant (critical ratio for T and AT 0.91; for T and VAT 0.90; for AT and VAT 0.08).

Table I justifies the following generalizations: (1) The A and V modes are not significantly different, although A has a slightly higher average of correct reproductions than V. (2) A and V modes are markedly superior to T. (3) All modes which contain T stimulation are inferior to those which lack T. (4) Pure modes (A, V, T) are superior to mixed modes. V and A are slightly superior to VA; T is slightly superior to mixed modes containing T. In a number of cases, however, the differences between pure and mixed modes are small and not statistically significant; but VA is significantly lower than A (critical ratio 5.24) and VT is significantly lower than T (critical ratio 4.23).

At the close of the experiment the Ss were asked: "Do you think the mode of presentation made any difference in the accuracy of your response?" Forty-eight of the Ss replied 'Yes,' two didn't answer. They were asked further to state which mode seemed the most difficult. Tabulation of the number of times a mode was mentioned as difficult gives:

A	0
V	0
VA	0
T	18
VT	18
AT	25
VAT	37

Thus reports from the Ss agree with the figures of Table I in the conclusion that modes which contain T are more difficult than those which lack T. The S's order of difficulty cor-

[16] Critical ratios for all differences were computed by dividing a difference by the P.E. of the difference. These values are tabulated in Dr. Compton's dissertation (see footnote 11).

responds fairly well with the average scores. The only obvious discrepancy is that the Ss rate VAT the most difficult whereas on the basis of scores VT is the most difficult.

The explanations given by S for the difficulty of T modes may be generalized as follows: (1) lack of practice in the use of the tactual sense; (2) difficulty in the shifts of 'attention' between tactual points on the table and the other points above the table; (3) confusion in the case of touch between the position of the stimulus-point and the position of the response key. Points 2 and 3 suggest that the relatively low scores for T may depend upon the spatial arrangement of stimulators and keys in the apparatus rather than upon any inadequacy of the tactual sense. To investigate this point a control experiment was made three weeks after the close of the main experiment.

Control Experiment upon Position of Tactual Stimulators. —Reference to Plate I reveals that the visual stimulus-points *A*, *B*, *C*, *D*, *E* and the auditory points *Q*, *R*, *S*, *T*, *U* are in approximately the same relative positions. The tactual points, *I*, *J*, *K*, *L*, *M*, on the other hand, are upon a different spatial level and also closer to S than the visual and auditory points. Further, the signal keys 1, 2, 3, 4, 5 are directly below visual and auditory stimulators and readily associated with them whereas the association between signal keys and the invisible tactual stimulators is not so obvious and direct.[17] Possibly the spatial plan of the apparatus is partly responsible for the result given in Table I.

To study the matter a change was made in the apparatus. Five lamps were mounted on the table as close as possible to the tactual stimulus-points; the lamps were just beyond S's arms but not in contact with them. The arrangement gave spatial congruity for V and T modes, comparable in every way to the congruity between V and A in the main experiment. There was no longer the necessity of changing bodily adjustment from upper to lower level and back as with the mixed T modes of the main experiment. The instructions were the same as in the main experiment except that reference to sound was dropped.

[17] Tactual stimulators and signal keys are on the same radii but these radii are not visible to S.

The 28 patterns of the main experiment were paired on the basis of difficulty and one member from each pair was selected to make a series of 14 patterns for the control experiment.[18] The 14 patterns were presented three times—in V, T, and VT modes. The 42 presentations were arranged in a single series to balance pattern-difficulty and to distribute practice among the modes. Thirty-two of the original 50 Ss gave an additional hour to the control experiment.

Results are summarized in Table II. The Table shows the average number of correct reproductions made by the Ss, the standard deviation for the distribution of scores, and

TABLE II

AVERAGE NUMBER OF CORRECT REPRODUCTIONS FOR VIS., TACT., AND VIS.-TACT. MODES OF PRESENTATION WITH STIMULATORS NEAR TOGETHER

Mode of Presentation	Average No. of Correct Reproductions out of 14 Presentations	σ_{dis}	P.E.$_M$
Vis.	9.22	3.29	.39
Vis.-tact.	7.94	3.34	.40
Tact.	7.69	3.12	.37

the probable errors of the means. The reproduction scores are higher for V than for VT and T modes. The differences are small but statistically significant (critical ratio for V and T 6.12; for V and VT 6.09). The difference between T and VT is not significant (critical ratio 0.94).

The control experiment justifies the statement that the immediate reproduction of patterns is superior when the patterns are presented visually than when the same patterns are presented in a T or VT mode. This confirms the conclusion of the main experiment that T and VT modes have lower scores than V.

Differences among the three modes are *relatively* less in the control experiment than in the main experiment. There are two probable explanations of this: (1) The elimination of two spatial levels and the necessity of shifting from one level to the other makes S's task simpler and removes one factor of difficulty in the VT mode. (2) The Ss in the control experi-

[18] These 14 patterns were Nos. 2, 3, 6, 7, 11, 12, 13, 17, 19, 22, 23, 24, 26, 27. See footnote 13.

ment are more highly practiced than in the main experiment. Evidence given below indicates that practice tends to equalize the differences among the modes.

The General Effect of Practice.—There were four experimental periods per S spaced exactly one week apart. Each period contained 49 presentations per S equally distributed among 7 modes of presentation and balanced as to pattern. The total number of correct reproductions for the group of 50 Ss is given below by successive periods.

Period	No. Correct	Percent Correct
I	741	30.2
II	994	40.6
III	1232	50.3
IV	1423	58.1

The curve for these four points approximates a straight line which indicates that the limit of practice is well beyond the performance of the fourth day and that the experiment as a whole deals with the earlier stages of practice.[19]

The Differential Effect of Practice.—How does the practice effect vary with the mode of presentation? Table III gives the total number of correct reproductions for the group distributed by experimental period and by mode. It should be remembered that there were only 7 patterns per period for

TABLE III

NUMBER OF CORRECT REPRODUCTIONS ON SUCCESSIVE PRACTICE PERIODS FOR DIFFERENT MODES OF PRESENTATION

Each mode was used 350 times per period.

Mode of Presentation	Period				Total
	I	II	III	IV	
Aud.	218	152	189	252	811
Vis.	179	200	169	246	793
Vis.-aud.	95	151	232	270	748
Tact.	79	83	149	224	535
Aud.-tact.	51	134	177	159	521
Vis.-aud.-tact.	60	132	185	143	520
Vis.-tact.	59	142	132	129	462
Totals	741	994	1232	1423	4390

[19] The present experiment confirms the previous one so far as practice effect is concerned. Reference footnote 6.

each mode and, since the patterns differed widely in difficulty (see discussion of pattern-difficulty below), a given selection of 7 patterns necessarily involved considerable variation in difficulty apart from practice. For the experiment as a whole, for each mode as a whole, and for each experimental period as a whole, the factor of pattern-difficulty was balanced; but for the individual scores of Table III it is an important unbalanced variable. Since we do not know how much variability is introduced by the selection of patterns, we must be cautious in interpreting results.

Making allowances for variation due to pattern selection certain gross relationships can be seen in Table III. (1) The A and V modes are consistently high throughout; variations from period to period (pattern-difficulty) obscure until the last period whatever practice effect may exist. (2) The VA and T modes are greatly inferior to the A and V on the first day but show a steady and marked increase of score with practice. On the last day VA has the highest score of all and T is not far below V and A. This indicates that with the unfamiliar T presentations as also with the unaccustomed alternations from V to A in the VA patterns, practice is an important factor determining the scores. (3) The three mixed T modes (AT, VT, VAT) are the lowest of all at the start; they show gains with practice but on the last day they are again relatively the lowest of all the modes. Doubtless the explanation here lies in a factor of difficulty not present in the other modes—the necessity of shifting adjustment from upper to table level. When this factor of difficulty was removed in the control experiment VT was no longer inferior to T (Compare Tables I and II). The factor in question does not exist with V, A, VA, and T.

Thus practice appears to have a differential effect. It affects least A and V modes and most T and the mixed modes. The modes which involve stimulation on two spatial levels are relatively inferior despite practice.

The control experiment offers another opportunity to study the differential effect of practice. Since the 14 patterns which were presented in each mode were paired in difficulty with the

remaining 14 of the main experiment, the averages can be multiplied by 2 for direct comparison with the averages of the main experiment. A comparison shows the following:

Mode of Presentation	Main Experiment Average No. Right	Control Experiment Average No. Right (× 2)	Gain
Vis	15.86	18.44	2.58
Tact	10.70	15.38	4.68
Vis.-tact	9.24	15.88	6.64

One point to note is that the averages in the control experiment are all higher than the corresponding averages for the main experiment. This is doubtless a general practice effect, for the Ss in the control had benefited by continued practice throughout the main experiment. Despite a change in position of the lamps the practice-gain carried over. It should be recalled that the averages for the main experiment are based upon four practice periods and include the early stages of practice along with the later stages. A second point to note is that the gain in the control experiment is least for V, greater for T, and greatest for VT. The elimination of two spatial levels with VT is responsible for some of the gain.

The above evidence for a differential practice effect is complicated by several uncontrolled factors. Yet it points consistently to the conclusion that practice tends to reduce the differences among the modes.

At the start of the experiment the tactual sense of the forearms was not practiced in the kind of discriminating and localizing reaction required by experimental conditions, whereas the eye and the ear were thoroughly habituated to the discriminations required of them. The Ss themselves mentioned unfamiliarity with the use of touch as a source of difficulty. It is not surprising, therefore, that practice makes the greatest difference with those modes of presentation which are least familiar, and that it tends to reduce the differences among the modes.[20]

[20] There are a good many studies dealing with the mode of presentation in relation to learning and memory. The work is mainly of pedagogical interest. H. L. Kock, Some factors affecting the relative efficiency of certain modes of presenting material for

Ability of S in Relation to Mode of Presentation.—If a S makes a high reproduction score in one mode of presentation, is he likely to make high scores in the other modes? Coefficients of correlation answer the question.

TABLE IV

COEFFICIENTS OF CORRELATION BETWEEN REPRODUCTION SCORES FOR DIFFERENT MODES OF PRESENTATION

	Aud.	Vis.	Vis.-Aud.	Tact.	Aud.-Tact.	Vis.-Aud.-Tact.
Vis	.90					
Vis.-aud	.88	.86				
Tact	.88	.86	.81			
Aud.-tact	.78	.80	.76	.81		
Vis.-aud.-tact	.80	.76	.79	.79	.89	
Vis.-tact	.81	.76	.78	.82	.87	.92

Table IV presents the coefficients of correlation between the different modes. The coefficients were computed by the product-moment method between the raw scores of the Ss. Every S was presented with the same 28 patterns in each mode and the number of these correctly reproduced constitutes the raw score for the mode in question. The coefficients show the extent to which an S making a high score in one mode tends to make high scores in other modes.

The values are consistently high. When corrected for attenuation they approximate 1.00 with a range from .87 to 1.08. In the control experiment described above, correlations were similarly computed with this result:

$$r_{vt} = .79, \qquad r_{v(vt)} = .86, \qquad r_{t(vt)} = .76.$$

The correlations show that if an S makes a high score in one mode of presentation, he is very apt to make a high score in all modes. In other words, making a relatively high score in the performance studied is to a large extent independent of the mode of presentation.

The performance involves the organizing of discrete im-

memorizing, *Amer. J. Psychol.*, 1930, **42**, 370–388; F. J. O'Brien, A qualitative investigation of the effect of mode of presentation upon the process of learning, *Amer. J. Psychol.*, 1921, **32**, 249–283; V. A. C. Henmon, The relation between mode of presentation and retention, *Psychol. Rev.*, 1912, **19**, 79–96.

pressions into a pattern on the basis of position and sequence and immediate reacting based upon this temporary bodily organization. The results indicate that a central factor is of prime importance in the performance, and sensory processes are of secondary importance.[21]

Reliability.—Since the experimental series had no duplicate, self-correlations between the half-series were worked out [22] and the reliability of the whole was estimated by Spearman's formula. Coefficients of reliability thus found were:

Auditory	.86
Visual	.89
Visual-auditory	.83
Tactual	.82
Auditory-tactual	.91
Visual-auditory-tactual	.86
Visual-tactual	.84
Whole experiment	.96

These values are consistently high. They are higher than most reliability coefficients found in memory and learning experiments.[23]

Pattern-difficulty in Relation to Mode of Presentation.[24]—The 28 patterns used in the experiment were selected on the basis of preliminary work to give a wide range of difficulty. In the main experiment each of these patterns was presented 350 times (once in each of the 7 modes to 50 Ss). Pattern No. 2 (345123) which contains positions in regular sequence proved to be the easiest with 290 correct reproductions out of a possible 350. Pattern No. 23 (213534) was the most difficult with only 42 correct reproductions out of the same number. Within this range was a balanced distribution of

[21] It was found that 19 of the Ss had taken the *Chicago psychological examination for high school graduates and college freshmen*, in connection with registration at the University of Illinois. Correlation between scores made on the test and scores in the present experiment was .42 ± .13.

[22] This was done by ranking scores in order of magnitude and selecting 'a' and 'b' scores according to the order: a b b a a b b a, etc., and then combining 'a' and 'b' scores into two independent series. This division was made independently for each mode and for the whole series of 196 items regardless of mode.

[23] J. A. McGeoch, Memory, *Psychol. Bull.*, 1928, **25**, 525–6.

[24] The term 'difficulty' designates a statistical fact: that the reproduction scores for some patterns are consistently higher than those for others. In analysing 'difficulty' the aim is to discover what conditions determine differences in score.

pattern-difficulty. The nature of pattern-difficulty will be considered presently but first let us turn to another question.

If a pattern is difficult in one mode is it difficult also when presented in other modes? To answer this question the 14 patterns used in the control experiment were ranked in order of score magnitude and coefficients of correlation were calculated by the rank order method. These correlations were:

$$r_{vt} = .96, \qquad r_{v(vt)} = .81, \qquad r_{t(vt)} = .85.$$

These values are very high and indicate that if a pattern is difficult in one mode, it is difficult also in other modes of presentation.

TABLE V

RANKING OF PATTERNS IN ORDER OF DIFFICULTY

Pattern	Control Series				Main Series Combined
	Vis.	Tact.	Vis.-Tact.	Combined	
345123	1	1	1	1	1
451235	2	3	2	2	4
512341	3	2	3.2	3	2
131245	4	5.5	3.2	4.5	5.5
545321	5	4	3.2	4.5	7
341235	6	7.5	3.2	6	5.5
514321	7	7.5	9	8	10
234531	8.3	5.5	3.2	7	3
432515	8.3	10	10.5	10	8.5
524135	8.3	9	13.5	11	12
245135	11	11	8	9	13
323514	12.5	12	10.5	12	8.5
452315	12.5	13	12	13	11
213534	14	14	13.5	14	14

Table V shows these 14 patterns ranked for difficulty. For comparison the rank of all modes combined in the main experiment is given. The Table shows at a glance that pattern-difficulty is relatively independent of the sensory channels through which a pattern is presented. Pattern-difficulty depends upon factors common to all modes.

The organism lives in a single world and reacts as a unit to spatial and temporal relations. *Getting set* is a process within a unitary organism. Environmental positions and sequences may be given by vision, audition, or touch; but in reacting to these presentations the organism's behavior depends much

more upon the organizing of spatial and temporal relations than upon the gaining of impressions through some particular sense channel.

Factors in Pattern-difficulty.—All patterns were ranked in order of difficulty on the basis of reproduction scores. The patterns were then diagramed so that the characteristics of easy and difficult patterns could be studied. The following factors in pattern-difficulty are worthy of note:

1. Simplicity of spatial pattern. In some cases the stimulus-points mark out a simple geometrical figure or design, in others complex; when the figure is complex the pattern is difficult.

2. Proximity of successive points. Patterns with successive points which are near together are relatively easy, probably because proximity favors the formation of groups.

3. Direction of bodily movement. The bodily movement which S must make to follow a sequence of stimulations is sometimes from left to right and sometimes in the reverse direction. The former, clockwise movement, appears to be easier, probably because of habituation to this type of movement.

4. Reversals in direction of movement. Some patterns involve movement in a constant direction; others involve repeated reversals from left to right and from right to left. The easy patterns have relatively few reversals.

5. Line-crossings in the spatial pattern. If the successive points of a pattern be joined by lines, the figure thus formed will sometimes have lines which cross each other; sometimes the patterns will be open. Difficult patterns have more crossings-over than easy ones. (Compare with points 1 and 4.)

6. Position in sequence. The central portion of a sequence appears to be more difficult than the beginning or end. Errors are more likely to be made in the center of a sequence.

The above analysis is incomplete and the factors mentioned overlap.[25] It would be instructive to make a further

[25] It is interesting to compare the above with the factors of advantage in the perception of figure and group as summarized by Woodworth in discussing observation. R. S. Woodworth, Psychology, revised 1929, 389.

study of pattern-difficulty in relation to the spatial and temporal arrangement of stimulus-points.

Discussion

Relation to Other Work.—The above experiment was planned as a study of organic set but it can equally well be regarded as a study of immediate memory. Most work on immediate memory has made use of letters, nonsense syllables, geometrical figures, words and other complex material which cannot be made equivalent when presented in different sense departments. In the present experiment the point stimulations in vision, audition, and touch were equivalent; each point marked a discrete position in space. Although the patterns presented differed in complexity and were presented in different sense departments, S's task was essentially unitary: to reproduce immediately the spatial sequence presented. Under the conditions we found that immediate reproduction was only to a slight degree dependent upon sense factors.

The span of immediate memory has been defined as the length of series which is just as likely to be reproduced as not.[26] The probability of S's reproducing a particular pattern undoubtedly depends upon the number of points in the pattern (length of series); but it depends also upon the speed of presentation, the amount of practice, the age of S, the spatial arrangement of the stimulus-points, the geometrical pattern made, etc. In the present study the patterns all had six points and the probability of correct reproduction varied widely from pattern to pattern. The easiest pattern was reproduced correctly 290 times out of 350 (probability of correct reproduction 0.83) and the most difficult pattern was reproduced 42 times (probability 0.12). The patterns were distributed evenly between these extremes. Pattern-difficulty is a variable factor in immediate memory which has not

[26] "The limen of immediate memory or the memory span is defined as that length of series which has the probability of 0.5 of being retained; in other words, that length of series which is as likely to be remembered as not." J. P. Guilford and K. M. Dallenbach, The determination of memory span by the method of constant stimuli, *Amer. J. Psychol.*, 1925, **36**, 626.

been expressed in terms of number. It is difficult to see how it can be treated quantitatively.

To make a measure of memory span, arbitrary experimental conditions have to be established; but there are so many conditions to control that the resulting 'span' is somewhat of an artifact.

The range of attention has been studied both with simultaneous and successive presentations but there is not yet agreement as to the 'range.' Gill and Dallenbach [27] concluded that the range of attention greatly exceeds the traditional limits; with 3 Os these investigators report limens ranging from 17 to 42 stimulus objects. Oberly,[28] comparing the spans of attention and memory, concluded that the 'attention span' limen ranges from 2.5 to 8, while the 'memory span' limen ranges from 6 to 13.5. In view of these divergent results it is plausible to assume that a measure of attention span is to a large degree a product of arbitrary conditions.

The memory-span and the attention-span-with-successive-presentation are closely related. It is said that memory-span involves a grouping of units whereas attention-span is measured in terms of non-grouped, discrete units. The distinction is difficult to apply in the present experiment. Some of the Ss reported grouping but others made no mention of it.[29]

The organizing of impressions suggests the formation of a physiological *Gestalt*. Our studies throw light upon some of the conditions which underlie the formation of configurations.[30]

Our work is also related to experiments upon the delayed reaction.[31] Some animals orient themselves towards the

[27] N. F. Gill and K. M. Dallenbach, A preliminary study of the range of attention *Amer. J. Psychol.*, 1926, **37**, 247–256.

[28] H. S. Oberly, A comparison of the spans of 'attention' and memory, *Amer. J. Psychol.*, 1928, **40**, 295–302.

[29] The manner of grouping was variable. Some Ss organized all six points into a pattern; others broke the sequence into groups of two or three; still others reported special groupings in the middle of the sequence. We found grouping previously, *op. cit.*, 269–75 (footnote 6).

[30] W. Köhler, Gestalt Psychology, Liveright, N. Y., 1929.

[31] For a critical review of the work on delayed reaction the reader may turn to the study of McAllister. Incidentally McAllister found that both visual and auditory stimulation were more effective for obtaining correct reactions after delay than the type of kinesthetic stimulation which he used; whereas visual and auditory stimulation were similar in effectiveness. W. G. McAllister, A further study of delayed reaction in the albino rat, *Comp. Psychol. Monog.*, 1932, No. 1.

stimulus-position and by means of a posture delay the reaction. Others depend upon internal cues rather than upon a gross neuromuscular adjustment. In the present experiment some of the Ss report successive orientations towards the stimulus-points—following the points with eye movements, nods of the head, or grosser bodily movements (kinesthetic processes). The reports, however, do not tell how the successive orientations get organized into a single pattern.

There was no true delay in the present experiment; reproduction of the pattern came immediately after the presentation. In some unpublished work we varied the interval between presentation and reproduction. Controlling the interval of delay placed the study in the field of true memory.

Summary and Conclusions

The process of establishing an organic set was studied by presenting six-point patterns to subjects by successive points, and calling for immediate reproduction of the pattern-sequence. The same patterns were presented in lights, in sounds, in touches, and in modes combining lights, sounds, touches (seven modes in all). Reproduction scores were studied and the following conclusions drawn:

1. The visual, auditory and mixed visual-auditory modes of presentation gave nearly equal reproduction scores. The tactual mode and the mixed modes containing tactual stimulation gave significantly lower scores.

2. Practice increased the reproduction scores for all modes. Practice had the greatest effect upon the pure tactual and the mixed visual-auditory modes; it had relatively little effect upon the pure visual and auditory modes which were consistently high. Practice increased the scores of the mixed tactual modes but they were consistently low.

3. The relatively low scores of the mixed tactual modes depended in part upon the spatial arrangement of the apparatus—tactual stimulators were on the table and visual and auditory stimulators at the head level. The differences between visual, tactual and visual-tactual modes were reduced in a control experiment when the stimulus-lamps were placed

on the table beside the touch stimulators. Visual presentation remained superior to tactual, but visual-tactual presentation was no longer inferior to tactual.

4. Coefficients of correlation between modes of presentation varied from .76 to .92 (from .87 to 1.08 when corrected for attenuation). These coefficients indicate that the establishing of a set depends upon some inner bodily process common to all sense departments more than upon the particular sense organs stimulated.

5. Reliability coefficients for the seven modes varied from .82 to .91. For the series as a whole the coefficient of reliability was .96.

6. The rank order for pattern-difficulty was similar for all modes. Coefficients of correlation varied from .81 to .96. The coefficients indicate that pattern-difficulty is not greatly dependent upon the sense department utilized in presenting a pattern but much more upon the spatial and temporal relations within the pattern itself.

7. Pattern-difficulty was found to depend upon: complexity of the spatial pattern, nearness of successive points in a pattern, direction of bodily movement (whether from left to right or right to left), reversals in the direction of movement, and other factors.

8. Establishing a temporary set is involved in immediate memory, in attending, in the formation of configurations, in the delayed reaction, as well as in other reactions. Our work, therefore,is related to a wide range of psychological problems.

(Manuscript received September 3, 1932)

TIME-ERRORS IN THE METHOD OF SINGLE STIMULI

BY CARROLL C. PRATT

Harvard University

When judgments with respect to some psychological dimension are made upon comparison-stimuli which are symmetrically distributed about the standard, and when the standard precedes these stimuli by three seconds or more, more judgments will be secured in the direction of increase than in the direction of decrease. The effective comparison-value or trace of the standard falls off or sinks. This so-called negative time-error has acquired renewed interest as the result of the elaborate treatment which Köhler has given the topic.[1] The matter relates directly to all psychophysical judgments, for even comparison of stimuli presented simultaneously may be regarded as the limiting case of temporal judgments. The ramifications of the subject reach far beyond psychophysics in the narrower sense. The course of any impression may be followed merely by varying the time-interval between its first appearance and its rearousal. The observer has simply to say whether the second impression is the same or different. For psychological purposes this type of judgment, because of its narrowness of determination, is far more reliable than generalized descriptions of fading images and memories. An extension of the technique for studying the time-error is already bringing with it a method for investigating the fate of mnemonic traces in general.

Until recent studies by the method of single stimuli appeared,[2] it was generally assumed that the asymmetry of judgments in the comparison of successive stimuli was due to the temporal relation which the second stimuli bore to the

[1] W. Köhler, Zur Theorie des Sukzessivvergleichs und der Zeitfehler, *Psychol. Forsch.*, 1923, 4, 115–175.

[2] I am using the terminology suggested for this method by J. Volkmann, The method of single stimuli, *Amer. J. Psychol.*, 1932, 44, 808 f.

first. The asymmetry may be compensated for statistically either by reversing the order of presentation or by using a standard above the midpoint of the series, but the enhancement of the comparison-value of the second stimulus is not thereby eliminated. This persistent addition of an increment to the second impression was therefore naturally thought of as due somehow to the fact of paired relationship, and was most frequently accounted for either in terms of a fading image of the first impression or the weakening of the after-effect of the first stimulus, both of which events would cause a preponderance of judgments in the direction of increase and a lowering of the point of subjective equality beyond the midpoint of the stimuli.

Any set of stimuli, however, which would ordinarily serve for purposes of comparison with a standard can be presented singly to an observer for judgment in terms of an absolute scale of values. No standard is given: simply one stimulus at a time. And the observer is asked to report, in terms of some previously arranged scale, whether the impression is great, small, or medium, as such impressions go. The method of single stimuli obviously has much to recommend it as a contrivance for the saving of time and labor. Of much greater importance is the fact that it yields a distribution of frequencies of judgment, at least in the field of lifted weights, very similar to that secured by the method of limits or the method of constant stimuli,—so similar, as a matter of fact, that the same preponderance of judgments towards the upper end of the scale puts in an appearance.[3]

In the methods using a standard stimulus it was this shift of judgments above the midpoint of the series that made it possible to detect the presence of the negative *time*-error. The continued use of the word *Zeitfehler* since Fechner reflects the half unconscious operation of theoretical bias. The very useful methodological accident of using two stimuli juxtaposed in time made the view eminently acceptable that time was the major factor producing asymmetry of judgment. But exactly the same phenomenon occurs with weights when only one stimulus at a time is presented for judgment by absolute impression. The term 'time-error' may not therefore be the most appropriate. Since however it is so familiar, and since the two phenomena undoubtedly

[3] E. G. Wever and K. E. Zener, The method of absolute judgment in psychophysics, *Psychol. Rev.*, 1928, **35**, 466–493; S. W. Fernberger, On absolute and relative judgments in lifted weight experiments, *Amer. J. Psychol.*, 1931, **43**, 560–578.

have some relation to a common factor, there can surely be no harm in retaining it to describe the preponderance of judgments in one direction or the other, no matter whether these judgments are relative or absolute.

Since explanations of the usual time-error have always involved the assumption of the fading of the impression caused by the first stimulus or the weakening of its trace, it presents something of a puzzle to find that the same thing happens when there is no first stimulus. If the effects of the first stimulus are responsible for the increment added to the second impression, what continues to produce the increment when these effects are no longer present? Wever and Zener propose an extension of Köhler's trace-theory.

"To explain our findings with the absolute method we need go but a step farther along the same line of theorizing; let us suppose that there occurs as the experiment progresses a gradual sinking of the general level that serves as the basis of absolute judgment. Or, in terms of our theory, there is a constant tendency for the absolute series as a whole to move downward in the intensity-scale. Obviously, the result of any such constant tendency would be to bring each succeeding stimulus into relation with an absolute series which is shifted downward with respect to the scale of physical intensities. And as the stimulus is judged with respect to an absolute series that is 'too low,' the results, in consequence, would show a predominance of judgments of 'heavy,' as is actually the case." [4]

This proposal is plausible enough as far as it goes. The point of symmetry of the judgments secured by the method of single stimuli applied to weights is indeed below the stimulus midpoint, which is the same thing as saying that "there is a constant tendency for the absolute series as a whole to move downward in the intensity-scale." Such is the fact, within limits. It will be shown later, however, that the point of symmetry (hereafter referred to as PS) is not always lowered, and that even when it is, it cannot be referred without further question to the sinking of the whole scale of absolute impressions nor to what Köhler would call the settling of the sediment of the neural traces.

Fernberger was also puzzled by the low position of PS in his absolute series.[5] He referred by way of possible explanation to Müller's theory of muscular set or fatigue,[6] and suggested the carrying over of this account from the comparative to the absolute time-error. Müller's theory quickly runs into difficulties if extended beyond weights. In weights, to be sure, fatigue might cause the second member of a pair to feel heavier. In sounds, however, where the relative time-error is equally marked, fatigue, if it operated at all, would cause the second member to sound softer. It was nevertheless Fernberger's suggestion about muscular set for weights, together with some evidence that the PS for pitches comes very close to the stimulus midpoint,[7] instead of below it, that furnished part of the reason for performing the experiments to be considered presently.

How the intraserial effects, which Fernberger draws upon for further explanation of the lowered PS, could bring about a negative time-error is not clear, at least to the present writer. Fernberger discovered "that all of the *O*s were, from time to time at least, employing a sort of intraserial relative judgment in the Absolute Series. For

---

[4] Wever and Zener, *op. cit.*, 488.

[5] Fernberger, *op. cit.*, 576.

[6] L. J. Martin and G. E. Müller, *Zur Analyse der Unterschiedsempfindlichkeit*, 1899, 117.

[7] S. R. Truman and E. G. Wever, The judgment of pitch as a function of the series, *Univ. Calif. Pub. Psychol.*, 1928, 3, 215–223.

example, if an absolutely heavy weight were followed by one lighter—but not too much lighter—this second weight was likely to be judged 'intermediate.' On the other hand, if a weight just judged 'intermediate' were followed by one which was lighter it was almost sure to be judged 'light.'" [8] If the event mentioned in the last sentence happened too often, the time-error would be positive, not negative. If the converse was also true, namely, that a heavier weight following one judged 'intermediate' was almost sure to be judged 'heavy,' the opposing tendencies, other things being equal, would cancel each other. But the other things were not equal: the values of PS in Fernberger's experiments were in many cases more than 8 grm. below the midpoint of the series.

The Method

In the experiments now to be considered the impressions upon which the observers were asked to base their judgments were produced by the impact of a hard rubber ball at the end of a pendulum-rod striking against a block of ebony. In order to secure a linear series of intensities, the values on the pendulum-scale were translated into equidistant units by means of the formula and table furnished by Fechner.[9] If the sound produced by the fall through 90° is taken as intensity 10, the five intensities 8, 7, 6, 5, and 4 of Series I would correspond very nearly to elevations 78, 72, 66, 60, and 53°. On the same scale, intensities 2.5, 2.0, 1.5, 1.0, and 0.5, the stimuli for Series II, would equal elevations 41, 37, 32, 26, and 18°. By the use of the same formula five equidistant stimuli for Series III between 2.0 and 1.0 were calculated.

Instead of using the categories 'loud,' 'soft,' and 'medium,' it was decided to use a scale of numbers, as is often done for absolute judgments of hedonic tone. It was assumed that nine numbers, 1 to 9, would offer the observer sufficient latitude for the discrimination of differences. (To the observer the number of differences often seems more than the number of stimuli.) Such a procedure relieves the observer of the discomfort of saying 'loud' when he really means to indicate only a hair's breadth above 'medium.' No. 6 could be used for such a small increase over 'medium,' while No. 9 could be reserved for an indisputable 'loud,' and so on throughout the whole scale. With respect to the trouble-

[8] Fernberger, *ibid.*

[9] G. T. Fechner, *Elemente der Psychophysik*, 1859, I, 180.

some category of 'medium,' (or 'equal' in comparative procedures) it was considered best to effect a compromise. The writer is fully convinced of the existence of real phenomenal equality and of impressions which would have to be labelled 'medium.' At the same time, numerous studies have revealed how equivocal these categories are. They may contract to include only a very limited number of cases, or they may expand to include carelessly reported equalities, doubts and uncertainties, and all sorts of lapses of attention. The following instructions were therefore given the observer.

"Following the ready signal you will be presented with a sound. You are asked to judge the intensity of the sound in terms of one of the following values: *one*, *two*, *three*, *four*, *five*, *six*, *seven*, *eight*, or *nine*. Use number *nine* for the loudest sound, number *one* for the softest, and the remaining numbers for intensities falling between these extremes. Reserve number *five* for those impressions which fall so directly in the middle of the series of intensities that you find it impossible to assign values either above or below."

Four observers, wholly inexperienced in psychophysical judgments, took part in the experiment, and are indicated by the letters *B*, *F*, *K*, and *M*. After a few preliminary trials to enable the observer to form an idea of the range of impressions, fifty judgments on each stimulus in Series I and in Series II were taken. An interval of seven seconds occurred between each presentation of a stimulus. For purposes of calculation by Urban's phi-gamma process, all judgments above No. 5 were regarded as 'greater,' those below as 'less,' and No. 5 itself as 'equal.' Since the experiment was not at all concerned with the problem of acuity of discrimination or with the size of limens, but simply with the question of the existence of time-errors as revealed by the values of PS, the 'equal' judgments were divided half to the upper and half to the lower categories, and the 50 percent point on either of the resulting curves calculated.

In work on hedonic tone the use of absolute scales involving more than nine members frequently reveals marked preferential values.[10] Certain members are seized upon more

[10] J. G. Beebe-Center, *The Psychology of Pleasantness and Unpleasantness*, 1932, 25–29.

frequently than would seem to be warranted by the gradated impressions. Figure 1 suggests that apart from the natural falling off at the ends, no marked preferential values occurred on the nine-membered scale used in this experiment. The relative positions of the two curves reveal the operation of

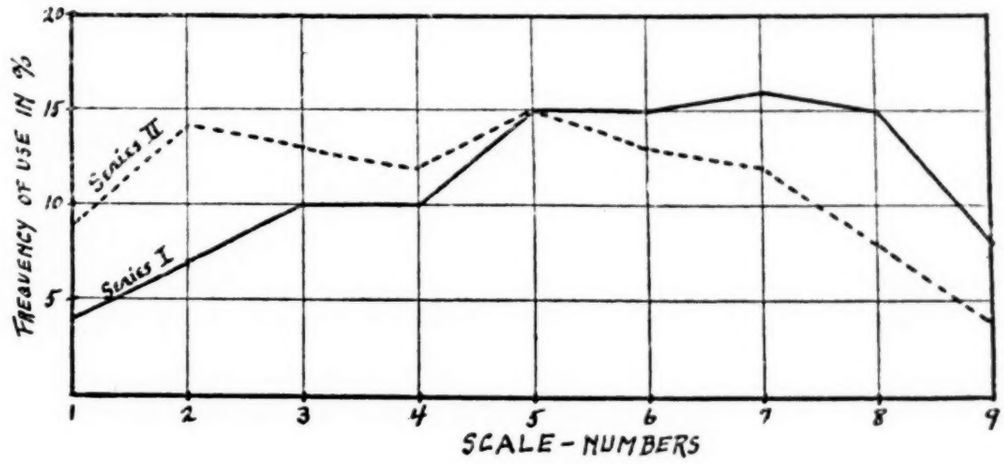

FIG. 1.

the time-errors which will be considered shortly. More high scale-numbers were used in Series I than in Series II, and *vice versa*.

Since the use of the method of single stimuli seems, at first sight, a radical departure from the usual comparative methods, it may be of interest to note the degree to which the curves approach normality. Urban suggests the sum of the squared differences of theoretical and observed frequencies as an index of goodness of fit. Thomson disapproves of this formula, and recommends the use of value P in Pearson's table No. XII.[11] Enough time was spent to discover that most of the present curves fall deplorably short of unity (perfect correspondence) in terms of Thomson's process. But Thomson also found that for Urban's experiments "the normal integral itself is an atrociously bad fit to the data in most cases."[12] In the present context it is sufficient to point out that the values of $\Sigma(P' - P)^2$ in Table I come close to zero (perfect correspondence by this process), and that

[11] G. H. Thomson, The criterion of goodness of fit of psychophysical curves, *Biometrika*, 1919, 12, 216–230.

[12] *Ibid.*, 229.

the data therefore are no worse than those secured by relative methods. The values also agree well with those secured for weights by Wever and Zener.[13]

TABLE I

VALUES OF $\Sigma(P' - P)^2$ FOR FOUR *O*s IN SERIES I AND II

	I	II
B	0.0018	0.0403
F	0.0023	0.0037
K	0.0013	0.0026
M	0.0135	0.0018

Table II gives the average scale-value assigned by each observer to each stimulus in Series I and in Series II. The averages of these values for the four observers are plotted against the stimuli in Fig. 2. The stimuli for both series, although not separated by the same amounts, are placed along the same scale merely to make clearer the choice by the observers of higher numbers in the first series than in the second. This difference again illustrates the operation of time-errors.

TABLE II

AVERAGE SCALE-VALUE ASSIGNED BY EACH *O* TO EACH STIMULUS IN SERIES I AND II

		B	*F*	*K*	*M*
I	8	7.40	7.86	7.18	7.62
	7	6.54	6.80	6.40	6.84
	6	5.94	6.00	5.70	6.06
	5	4.68	4.86	4.10	4.72
	4	3.14	3.14	3.12	3.26
II	2.5	6.92	7.26	7.10	7.16
	2.0	5.66	6.10	5.74	6.06
	1.5	4.38	4.98	4.30	4.44
	1.0	3.38	3.52	2.90	3.16
	0.5	2.10	2.02	2.26	1.84

CONSIDERATION OF RESULTS

The stimuli for Series I were 8, 7, 6, 5, and 4. These values were not chosen for any special reason. They were merely convenient to work with and gave a good distribution of judgments. Long before the series was finished it was apparent that the results were going to show negative time-

[13] Wever and Zener, *op. cit.*, 482.

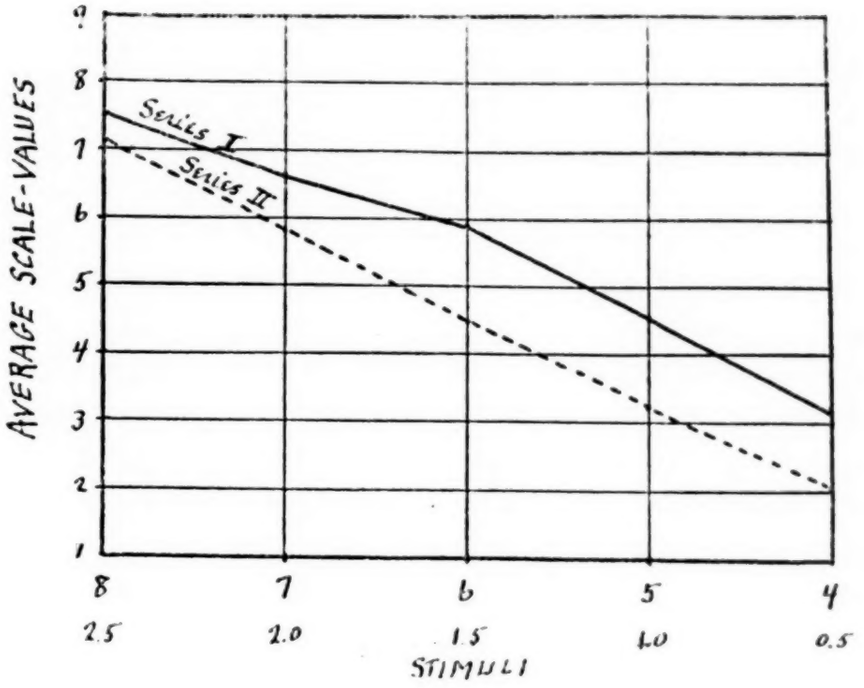

FIG. 2.

errors for each observer: the top stimuli were receiving more high numbers than the bottom stimuli low ones. Already one of the questions which the experiment was designed to answer was tentatively settled. Just as in experiments with lifted weights, so with sounds, the PS of the method of single stimuli is below the midpoint of the stimulus-series. This fact is clearly seen in Table III. The middle stimulus for

TABLE III

VALUES OF PS AND D PERCENT FOR SERIES I

	B	*F*	*K*	*M*
PS.....	5.36	5.24	5.50	5.30
D percent....	−0.236	−0.292	−0.164	−0.236

Series I was 6. For each observer the value of PS is below 6. D percent is equal to $(l - g)/n$, where l equals the number of judgments to which scale-values less than 5 were assigned and g the number to which values greater than 5 were assigned. A negative value of D percent indicates a negative time-error, a positive value a positive time-error.

If the experiment had been stopped at this point, it would

be in order to raise the same question which others have asked who have noted the lowered PS with single stimuli. Why should stimuli *without* a standard produce the phenomenon which invariably has been thought of as due to the action of the standard? The most plausible answer would probably involve an expansion of Köhler's theory to the extent of assuming that the trace of the whole series of stimuli, acting as a unit, falls off or decreases. Each presentation is thereby brought into relation with a slightly lowered level which serves as an unconscious standard of reference. The experiment, however, was not stopped at this point. Other considerations made it seem desirable to carry on further.

Lauenstein has recently suggested that Köhler's theory of a sinking trace should be given up in favor of one based on the principle of assimilation.[14] The trace of an impression does not sink; it is brought into relation, by a process of assimilation, with the traces of other impressions which stand in close structural connection with the impression upon which a judgment is made. If sounds, *e.g.*, are compared against a loud tonal background, a positive instead of a negative time-error results. The trace of the standard in this case assimilates to that of the background, thus acquiring an increased effective comparison-value and producing a preponderance of judgments in the direction of decrease. The present writer has adduced experimental evidence to show that Lauenstein has over-generalized.[15] When traces or impressions in the background are in close enough connection with those which form the basis for judgment, then it may be said that assimilation operates. But such a situation constitutes a very special condition, except in vision. For the usual psychophysical judgment the background is empty. For weights and sounds, at all events, the background is phenomenally empty, unless one deliberately thrusts something into it. And experiment shows that a phenomenally empty background does not stand in close enough connection with the impressions upon which judgment is being passed to permit of assimilation to these impressions.

Whenever assimilation cannot take place, it is a safe assumption that the after-effect of the standard begins to subside. Negative time-errors in psychophysics are too common to destroy credence in a gradually diminishing after-effect from a standard stimulus.[16] If however there were no standard, the single impressions in the method of

[14] O. Lauenstein, Ansatz zu einer physiologischen Theorie des Vergleichs und der Zeitfehler, *Psychol. Forsch.*, 1932, **17**, 130–177.

[15] C. C. Pratt, The time-error in psychophysical judgments, *Amer. J. Psychol.*, 1933, **45**, 292–297.

[16] Another even more cogent reason, it seems to me, for not discarding the notion of a sinking trace comes from the field of memory. Memory is notoriously fallible. Even if from Hume down there were not the almost universal assent to the faintness of the image as contrasted with the vividness of the impression, the gaps and untrustworthiness of memory would lead to some theory about disintegrating residua. The negative time-error may well be regarded as a special case of memory.

single stimuli, floating about, as it were, with nothing to attach themselves to, might then be brought into relation with more remote trace-formations not necessarily having to do with the immediate experimental situation. In the relative procedure the persistent repetition of the standard would blot out any such traces, but without a standard they might come into play. It was this hypothesis which led to the experiments in Series II. It was considered entirely possible that the lowered PS of Series I was not due to the sinking of the traces of the stimuli, but to the action of more remote traces which were themselves below these stimuli. If this were true, another set of stimuli still lower than these traces might produce a positive time-error.

The stimuli for Series II were 2.5, 2.0, 1.5, 1.0, and 0.5. These stimuli were purposely chosen because they made considerably softer sounds than the ones in Series I. The observers were given exactly the same instructions for this series. Before the series was completed it was evident that the results were going to show positive time-errors for each observer: the bottom stimuli were receiving more low numbers than the top stimuli high ones. Already one of the questions which the experiment was designed to answer was partially settled in the negative. Not all sounds, when presented for judgment by the method of single stimuli, yield a PS below their midpoint. This fact is apparent in Table IV. The

TABLE IV

VALUES OF PS AND D PERCENT FOR SERIES II

	B	*F*	*K*	*M*
PS.....	1.54	1.54	1.67	1.67
D percent....	0.024	0.032	0.136	0.140

middle stimulus for the series was 1.5. For each observer the value of PS is *above* 1.5, and the values of D percent are all positive.

The appearance of positive time-errors in this series would seem to verify the above hypothesis. The conditions for the two series were identical, with the exception of the intensity of the stimuli used. It is not clear how this difference in intensity, when subjected to the same scale of judgment, could bring about a lowered PS in one case and a raised PS in the other. Why should the traces of one region of intensity sink and those of another rise? Some other factor must be at work. It is suggested that this factor, however vaguely

envisaged, has to do with a general level of reference built up in the past which serves as a basis of alignment for all impressions constituting a more or less homogeneous mass. Intensities of sound would surely be a homogeneous group. How long prior to the presentation of the impressions which are judged with reference to the general level the level itself is built up and established is difficult to say. It may develop during the experiment proper, or it may be a more or less fixed affair which the individual has acquired through long experience. The former alternative seems indicated by recent experiments of Woodrow.[17] The latter possibility must nevertheless exist for many types of psychological experience which individuals think they are judging in absolute terms. Beebe-Center in his work on hedonic contrast found evidence for the operation of a level of reference which had been formed by secondary stimuli presented two weeks prior to the primary stimuli.[18] He does not venture to predict the time-span of such a level, but suggests that it might very well persist indefinitely until broken up by contrary groupings. The matter is one which clearly deserves further experimental investigation.

At all events, when the sounds, in the present situation, are above the general level of reference, they receive a still further enhancement in the louder direction; when below, they suffer a still further diminution in the softer direction. It is this fact which makes it impossible to speak of assimilation. Assimilation means the drawing together of whatever items are affecting each other. When an item is judged in the direction opposite to the one having an influence upon it, the process is generally referred to as *contrast*. The action of contrast is presumably of a complicated and varied nature. The present experiment reveals only one aspect of its operation. Other investigators have turned up related phenomena in psychophysics and also in hedonic tone.[19]

[17] H. Woodrow, Weight-discrimination with a varying standard, *Amer. J. Psychol.*, 1933, **45**, 391–416.

[18] Beebe-Center, *op. cit.*, 229–234.

[19] H. L. Hollingworth, The central tendency of judgment, *J. Philos.*, 1910, **7**, 461–468; Fernberger, Interdependence of judgments within the series for the method of constant stimuli, *J. Exper. Psychol.*, 1920, **3**, 126–150; W. D. Turner, Intra-serial effects with lifted weights, *Amer. J. Psychol.*, 1931, **43**, 1–25; Beebe-Center, *ibid.*

In all strictness it is not even entirely correct to draw upon the principle of contrast in the above situation. It is merely a manner of speaking, to indicate a directional tendency away from the factor influencing the judgment, and as such is legitimate enough. It helps one to keep in mind the direction which the judgments take. A very simple schematic illustration will make it clear, however, that contrast may not be operating at all.

The middle stimulus for Series I was 6. The PS for the series was below 6. The assumption that this negative time-error can be accounted for by the sinking of the traces of the whole series of stimuli is negated by the fact that the PS for Series II was *above* 1.5, the middle stimulus of the series. The factor which would produce a negative time-error in the first case and a positive in the second would be a general level of standard reference lying *between* the two series. Let us suppose, merely for the sake of illustration, that the value of this general level were 3. In Series I all of the judgments would be influenced to some extent by the unconscious standard, 3. Since the stimuli of this series were all above 3, they would naturally receive an enhancement in the direction of increase, not by contrast, but by the very fact that they were all louder than 3. The reason the stimuli were not *all* judged greater or louder is obviously because each impression was judged largely in its own right, or rather, in relation to the surrounding impressions of the series. The unconscious standard of reference not only does not obliterate such impressions completely, it influences them only to a slight extent,—just enough to produce more 'loud' judgments than one would expect. The overproduction of 'soft' judgments in Series II is explained by working the same principle the other way around.

If contrast operates anywhere, it operates in the situation which Lauenstein accounts for in terms of assimilation. In his experiments, as well as the experiments of the present writer referred to in footnote 15 above, the perceptual field contains more than the two stimuli presented for judgment. The two stimuli are judged against very much louder or softer stimuli in the background. In the case of the loud background, a positive instead of a negative time-error appears. The comparison-stimuli suffer a diminution in phenomenal intensity. Lauenstein explains this fact by assuming that

the traces of the standard and of the background assimilate, thus producing a higher level for judgment and a preponderance of reports in the direction of decrease. It is fully as reasonable to suppose that the moderately loud comparison-stimuli, *by contrast with the louder phenomenal background*, are judged less intense than would be the case under normal circumstances. One has no direct evidence whatsoever about assimilation of traces. Psychological contrast, however, is a very frequently observed phenomenon. It is not always safe to suppose that a psychological fact is made scientifically more solid by surrounding it with speculative physiology. This matter will be touched upon briefly in closing. It still remains to discuss the results of Series III of the present experiment.

The appearance of a positive time-error, under the usual conditions of comparative judgments, is rather rare, whereas the literature of psychophysics is full of references to negative time-errors. So consistently do they appear, as a matter of fact, that it is common practice deliberately to compensate for them either by reversing the order of presentation of the stimuli or by using a standard stimulus which is above the midpoint of the series. For lifted weights, *e.g.*, a stimulus of 100 grm. usually serves as the standard for the comparison-stimuli 88, 92, 96, 100, and 104. Now if an unconscious standard of reference is always potentially present, why has it not made its appearance more frequently in the form of positive time-errors for stimuli low down upon the intensive continua? The answer is not far to seek. An unconscious standard can only function when a very real phenomenal standard (or its immediate trace) does not blot it out. In the comparative procedure a standard stimulus is introduced with every presentation, and the observer naturally judges the comparison-stimuli in terms of the standard. After an interval of two or three seconds the trace of the standard almost always begins to sink, unless unusual conditions or long practice-effects are introduced.[20] If, then, a standard is always given first, and if the trace of the standard has begun to sink before the comparison-stimulus appears, the prevalence of negative time-errors is thereby easily explained.

If this argument is sound, it ought to follow that if a standard stimulus were introduced before each presentation

[20] The course of a negative time-error with the passage of time is a devious one. It may even reverse itself and become positive. Dr. J. G. Needham will shortly publish the results of work done along this line in the Harvard laboratory.

of the comparison-stimuli of Series II, the positive time-error would change to negative. This is exactly what happened in Series III. In this series it was found necessary to make the difference between the stimuli (2.00, 1.75, 1.50, 1.25, and 1.00) smaller, but the midpoint 1.5 was the same as in Series II, and the standard stimulus, presented four seconds before the comparison-stimulus, was also 1.5. The observers were now told of course to compare the second impression with the first. Unfortunately there was not time to run through more than 25 judgments on each stimulus, so that the results may lack a certain statistical reliability. But their general trend is unmistakable. The points of subjective equality, calculated by Urban's process, and D percent are given in Table V.

TABLE V

VALUES OF PSE AND D PERCENT FOR SERIES III

	B	*F*	*K*	*M*
PSE.....	1.39	1.38	1.48	1.31
D percent....	−0.192	−0.176	−0.032	−0.240

Both of these values indicate negative time-errors for each observer, whereas for the stimuli of Series II without a standard the values were positive. The judgments in this relative procedure were apparently being made not in terms of an unconscious standard located above the stimulus-series, but in terms of a constantly repeated standard stimulus whose trace was beginning to sink just before the comparison-stimuli appeared,—hence the reversal of the direction of the time-errors.

In conclusion a word should be said about the use of the term 'trace.' In the present context the word is not intended to involve any entangling alliances with physiology or with physical chemistry. It refers merely to an after-effect of a stimulus, an after-effect which is known directly through the absence of perfect correlation between the time-relations of the stimulus and those of the organism. The response lasts longer than the stimulus, or the phenomenal correlate of the stimulus persists after the stimulus has been removed, or

a subsequent impression is different from what it would have been had there been no previous stimulus. The word 'trace' is used simply to avoid such circumlocutions as "a subsequent impression is different from what it would have been had there been no previous stimulus." One may envisage a trace in any way that he pleases. Physiology is one way. Physical chemistry is another. And a third no less effective way is in terms of stimulus-report correlations.

Köhler in his work on the time-error has written a very pointed and impressive polemic against those psychologists who are sceptical about the value of hypotheses making use of physical chemistry in the field of brain-physiology.[21] He apparently regards such hypotheses as the only ones having any significance for psychology, or at all events he fails to mention the possibility of other sorts of hypotheses, such as those generated out of, while still remaining in the field of observed interrelations. Why must psychological hypotheses be limited to physical chemistry? J. S. Mill allowed the scientist much greater freedom. "An hypothesis being a mere supposition, there are no other limits to hypotheses than those of the human imagination."[22] Mill hastened to add, however, that the best hypotheses are those which make use of concepts applicable to the observed events.

Köhler examines two hypotheses with respect to the negative time-error in considerable detail and elaborates them in terminology which derives from physical chemistry. Very simply stated, the two hypotheses follow. (1) The after-effect or trace of the first stimulus may attach itself to the second stimulus, thus conferring upon the latter an additional increment of intensity. (2) The trace may not attach itself to the second stimulus. It may instead retain relative independence and furnish the lowered level against which the second stimulus is judged. Both of these hypotheses, as stated, account equally well for the gross fact of the negative time-error. And neither of them, be it noted, requires the tiniest fragment of physical chemistry or of physiology for its invention or formulation. Two consequences, argues Köhler, may be deduced from the speculations about the dynamics of the brain field which gave rise to the hypotheses. These consequences are incompatible and therefore offer a crucial problem for experiment to settle. From the first hypothesis it follows that with a longer time-interval between the standard and comparison the after-effect of the first stimulus upon the second should be weaker, thus returning the distribution of judgments nearer to symmetry. From the second hypothesis it follows that with a longer time-interval the trace of the first stimulus should be lower, thus creating a still greater asymmetry of judgment. Experiment showed that the latter alternative was the true one. Hence Köhler abandoned the first hypothesis and clung to the second one, until Lauenstein revised it under the elaborate physiological formula of trace-assimilation. Again it must be noted that the deduction of these consequences requires no concepts other than those already implied in the stimulus-report relationships. The method of extending the time-interval was already in use. It was already known that the asymmetry of judgments was thereby increased. What was left, then, for the second hypothesis to offer in the way of new deductions? A sinking trace,

[21] Köhler, *op. cit.*, 139–142.

[22] J. S. Mill, *A System of Logic*, 1843, Bk. III, Ch. XIV, par. 4.

moreover, or a settling of a sediment, is nothing more than a parallel to Fechner's old idea of a fading image,—a better way of stating the case, to be sure, but only because in many comparison-judgments no imagery is present.

The lack of any new developments over a period of years in the field of psychophysical time-errors was not due to the absence of modern knowledge about physical chemistry. It was due rather to the fact that psychologists in their efforts to secure a measure of sensation free from error tried in every way to get rid of the time-error. Köhler deserves every possible credit and praise for bringing the problem back into psychology in its own right and for suggesting and making numerous experimental attacks upon it. The ramifications of the problem are numerous and of first rate importance in other fields than psychophysics. The conviction is hard to escape, however, that the problem in its present state derives small help from hypotheses regarding the physico-chemical action underlying the activities in the cortical centers.

The word 'trace' then need not connote anything more than certain functional relations between stimulus and judgment. It refers to the persistent after-effects of stimulation after the stimulus has been removed. Speculation as to what the various effects of a trace may be should be allowed to go in whatever direction seems profitable. For certain kinds of sensory phenomena physiology is certainly the place to go. For other kinds, physiology offers little assistance. It is sometimes even dangerous, for it lends a sense of false security because of its closer affinity with the more exact sciences. Psychology has its own problems and its own methods, and should have its own hypotheses, which need not in every case be physiological. The basic nature of a trace is beyond any reasonable question physiological. To say as much is to indulge in a superfluous confession of faith.

Summary

1. Judgments of intensities of sound are easily made in accordance with the procedure of the method of single stimuli.

2. The intensities may be judged very satisfactorily in terms of a scale of numbers. The middle number of the scale may be reserved for 'medium' judgments, thus narrowing the range of these frequently equivocal judgments.

3. The psychometric functions secured by this method compare favorably with those secured by the relative procedures.

4. Time-errors in this method tend to be negative at the upper end of the intensity-scale, and positive at the lower end.

5. The judgments in this method are relative in the sense that they are determined by the relation of each stimulus to the other members of the stimulus-series.

6. To a measurable extent the judgments are also relative to an intermediate level or standard of reference established by traces of past stimuli which form a homogeneous group with the present stimuli.

The following generalizations may be made with respect to time-errors in general. They must be regarded as tentative, for numerous important experimental variations which might upset the stimulus-report relationships upon which they have been based remain still to be worked out systematically.

1. The trace of the first member of a pair of stimuli presented for judgment with respect to intensity decreases in magnitude after the first three seconds, thus shifting the intensity of the second member upwards.

2. When an impression from some closely related secondary stimulus enters the comparison-field, the intensity of the second member is shifted in the direction opposite to that of this impression.

3. High intensities, when judged on an absolute scale, are shifted upwards; low intensities, downwards.

(Manuscript received August 21, 1933)

THE CONSTANCY OF GRAY WITH CONSTANT AND WITH CHANGING ILLUMINATION

BY C. OLIVER WEBER

Wells College

I. Introduction

The following experiments constitute a study of *Farbenkonstanz* under an entirely new and important condition, the condition of changing illumination. *Farbenkonstanz* (hereafter symbolized as *Fk.*) refers to the phenomenon, for which Helmholtz already offered an explanation, that colors and grays tend to remain phenomenally constant, despite changing illumination. Two white pieces of paper, one in daylight and the other in deep shadow, look remarkably alike to the eyes. But a photograph of them shows the one in shadow as vastly darker. To be exact, the shade which the one in shadow exhibits to the eye lies between the shade it would have in normal daylight and the darkening influence due to the shadow.[1] The phenomenon of constancy appears in hearing, touch, and in the other senses, but has been studied most thoroughly in the case of vision. The following experiments were planned and carried out in the Rostock Institute of Psychology.[2] The new and important feature of the experiments suggested by Professor Katz is the comparison of *Fk.* as it appears under constant illumination with *Fk.* as it appears under rapidly changing illumination. Now, in daily experience we very seldom see illumination changing constantly and rapidly. But, if *Fk.* nevertheless appears under this condition it should indicate that it is a native capacity and

[1] There are experimental conditions under which the darkening influence due to the reduction of light is over-compensated. See the study of Burzlaff cited below.

[2] I take this early occasion to thank the members of the Rostock Institute for the generous and intelligent aid which they constantly gave me, a foreigner in their midst, in this experiment. I owe a special debt to Professor Katz for conferences on the final results of the experiments, and to Professor Keller for constant help throughout the experiments.

not a consequence of individual experience. In the second section the experimental procedures and conditions are described. The third section gives and discusses the main results. A fourth section gives the bearing of results on theories of color constancy, especially in so far as they are concerned with the rôle played by illumination.

II. The Experiments

In order to avoid a misleading result because of its dependence on special methods of experimentation, we adopted, like Burzlaff, four different experimental settings.[3] Certain features were common to all four conditions of experimentation and these will be described first. The method of constant stimuli obtained in all four experiments. In all cases the observer (O) saw a shade of gray in the observation field for a period of about one second. This standard gray was presented two times, with an intervening pause. After the second exposure, O seeks an equivalent gray from a table of comparison grays made up of 30 shades in the order from the purest white to *Tuchschwarz*. The time taken for finding an equivalent gray was not limited. If O found it impossible to fix upon some one comparison gray he was permitted to give a range of grays within which the standard gray is regarded as belonging. In each one of the four experiments, the O's made observations under four conditions of illumination, as follows: (1) with illumination changing from dark to light, (2) from light to dark, (3) with the light constant at the bright extreme, and (4) with the light constant at the dark extreme. Each O made five judgments of each standard gray taken in chance order, for each condition of illumination. But the four conditions of illumination were taken in the order given above. The range of brightness of the changing light was determined by the white-black color-wheel values of the background upon which the grays appeared. At the dark extreme of illumination, the field corresponded to 24 degrees of white and the bright extreme gave a background 128 degrees white. This means, for instance, that a color wheel made up of two discs, pure

[3] W. Burzlaff, Methodologische Beiträge zum Problem der Farbenkonstanz, *Zeits. f. Psychol.*, **119**, 1931, 177–235.

white and *Tuchschwarz*, has to be set at 24 degrees white and 336 black to match the background given by low illumination. This absolute range of illumination, however, holds for but one particular gray, that of the background. If we state this range in relative terms, in terms of a ratio, it will have general application throughout the experiment. This method of measurement is standard in the literature on the subject. It is approximate, but the principle of constancy is marked by gross effects, and needs no highly refined method of measurement. The range of illumination lying between the bright and dark points in this experiment are approximately expressed by the ratio 5 : 1 (128/24).

The change of illumination is large, being a change from twilight to somewhat less than average daylight. Moreover this change is accomplished in the period of one second during which the observer never saw the standard gray in constant light. Under such conditions one might well suppose it utterly impossible for the eyes to gauge correctly the true shade of gray presented. The difficulties of observation are further increased by the fact that the rate of change of illumination is not uniform, but accelerated and decelerated, like the movements of a pendulum. This will be clear from a description (to follow) of the apparatus used. The observer uses both eyes, and dark adaptation is prevented by the fact that the dark room in which the experiments were carried out received a small but constant illumination from the day-light bulb that illuminated the table of comparison grays. It is necessary to take into consideration a momentary as opposed to a delayed adaptation of the eyes. The conditions of momentary adaptation have been quantitatively determined by Noldt.[4] Noldt's general procedure was as follows: The O's were dark-adapted for 15 minutes, after which they kept the eyes on a small fixation light, *b*. Suddenly, another light *a* appears, surrounding the fixation light, which disappears again after a measured interval of time. The time taken for the fixation light to become visible again is carefully measured with the Hipp Chronoscope. Momentary adapta-

[4] Fritz Noldt, Über Momentadaptation, *Zeits. f. Psychol.*, 1925, **97**, 32–61.

tion time, thus measured, is found to vary with certain conditions, with the intensities and areas of the two lights, and on the length of exposure of the *a* light and on the mode of its extinction. The *a* light was sometimes gradually extinguished, the period of extinction being one second. This corresponds to the second mode of illumination employed by us. But the range of illumination intensity extinguished in Noldt's experiment was far greater than in ours, hence his figures have no special application to our experiment. We were in a predicament in determining the extinction time. If this time is too long the O's will be led to judge the standard for the first or perhaps for the last portion of the extinction time, and will not be influenced by the extinction period as a whole. On the other hand, if the extinction is too rapid, it will be followed by an undesirable blind interval, the interval of momentary adaptation. The final extinction time of one second used by us was satisfactory. Only one observer (Be.) noted such a blind interval, and this only in Experiment I, which however was too brief to effect results seriously, since her results are on the whole as good as the rest.

We turn now to a brief description of the apparatus and of the experimental setting. The experiment took place in a dark room. The standard and comparison grays were made from Hering Grays (Zimmerman) supplemented with grays made by Burzlaff in the Rostock laboratory. *Tuchschwarz* was added to the list. The gray papers used were, except in Experiment I, six cm. square. Thirty grays arranged in order from white to black, in six rows of five grays each, served throughout as comparison stimuli. The shade of each gray was determined by the familiar color-wheel method, as described above. The measures thus obtained, only the white component being recorded, were further corrected on the basis of previous determinations which show that *Tuchschwarz* of 360 degrees still contains about six degrees of white. The brightness range of the 30 comparison grays as thus determined vary from 350 degrees white to 6 degrees white, that is, to *Tuchschwarz*. Five standard grays were selected whose white values were 265, 104, 63, 42, and 17 degrees. Both standard and comparison grays were presented on the same plane, the observer facing them at a distance of 180 centimeters.

Each of the standard and comparison fields were lighted by a 60 Watt daylight bulb from a distance of 114 centimeters, it being unfortunately the case that daylight cannot be controlled with the nicety required by experiments of this type. The red light which appears when the current supplying the bulb is very weak was avoided in this experiment, at least remained below the thresholds of observation. The bulbs used gave a slight excess of yellow light, but this difficulty is somewhat mitigated by the fact that it affects both standard and comparison fields. The O's view of the standard field was cut off by a vertical screen provided with a shutter by means of which the exposures were made. We have now to describe the apparatus used for varying the illumination

of the standard field. F. Noldt [5] had secured the gradual reduction of illumination by putting an electric light in circuit with a motor. When the current is first turned on the light is at its brightest, but as the motor gains speed it takes the current from the lamp and thus gradually reduces its candle-power. But it is not easy to secure a gradually increasing illumination by this means. We therefore employed a different device. The current lighting the bulb for the standard gray was passed through a long resistance coil provided with a sliding contact which altered the resistance continuously. This sliding contact was moved by a cam shaft driven by a large wheel on an electric motor. But the backward and forward motion imparted to the end of a shaft, the other end of which describes a circular motion, is accelerated and decelerated motion, and the light changes accordingly.

We come now to the features regarding which the four experiments differed from each other. They vary from conditions of complete reduction to conditions of high structurization (*Gliederung*) of the field. The field is reduced in our experiment by viewing it through a small aperture, which completely excludes the surroundings of the stimulus object to be judged. Reduction obtains only in the first experiment. The three remaining experiments vary in the degree of complexity of the standard field. Under conditions of reduction the surface of the stimulus object undergoes certain phenomenal changes, altering from surface color (*Oberflächenfarbe*) to film color (*Flächenfarbe*). At the same time constancy ceases to exist. The present study is not concerned with these facts, now generally established.[6] We are interested in the special problem of the rôle played by illumination in the phenomenon of constancy.

In Experiment I, O looks through a dark tunnel (*Dunkeltonne*) with a small round aperture at the further end through which the standard gray is seen. To prevent dark adaptation of the eyes the inside of the *Dunkeltonne* was faintly lighted during the intervals between exposures. In this experiment large sheets of the standard grays were used so that the observer saw the hole through which he looks completely filled with the standard gray, but of an area corresponding to that of the other three experiments. In Experiment II a simple degree of configuration appears. The observer looked through a short tube with round apertures at each end, directed towards the standard grays, these grays being 6 by 6 cm in size. O sees, not only the standard gray, but a small area of the ground. Experiment II differs from the others in that but three of the five standard grays were used, namely the two end values and the mid value (265, 63, and 17 degrees respectively.) In Experiment III O sees the gray square with no obstructions save the edges of the shutter window, this window being 10 by 15 cm in area. This aperture enables O to see the standard gray, all of the background, and even parts of the room where the background was placed. In Experiment IV the complexity of field of Experiment III is further increased by the introduction of 29 comparison grays which are arranged on the standard background as in the field of 30 comparison grays, except that the grays are in mixed order. The standard gray was presented in the third space in the second row from above. A thumb tack under the standard gray enabled the observer to isolate it as the gray to be judged. The situation here presented to the observer, through the shutter of the screen before him, comes the nearest to resembling daily life, with its disorderly arrangement of light and shade.

The above order of experiments, from complete reduction to rich configuration of the field, is not the time order in which they were given. Experiment I came third in

[5] F. Noldt, *op. cit.*, 36–41.

[6] See the systematic account in D. Katz's Der Aufbau der Farbwelt, especially Chapter II. This is the new (1930) edition of Die Erscheinungsweisen der Farben, 1911.

time, Experiment II was second, Experiment III was first, and Experiment IV was last. Thus, Experiments III and IV, representing the two extreme conditions of complete reduction and complete structurization, came after the other two experiments.

Four observers served in each experiment, except in Experiment II which had 3 observers. Three of the observers (Ke., B., and W.) served in all four experiments, and their results will be especially depended on for comparative purposes. The observers were: Professor David Katz (Ka.), Professor Hans Keller (Ke.), Herr Brückner (Br.), Frau Becker (Be.) and Mrs. Weber (W.). The first two mentioned were trained observers in the field in question, and the last three relatively untrained.

III. General Results of the Four Experiments

Our chief interest concerns the comparison grays which the O's selected as equivalent to the standards. Secondly, we give attention to the average deviations of each O from his or her own average choice of comparison grays for each standard gray. In cases where the O gave a range of comparison grays, we used, in our calculations, the mid-points of such ranges. Further, we computed the average extent of such ranges for the first and the fourth experiments and for the three O's taking part in them. These average ranges, which we may call the subjective deviation, will be the third problem to be considered. Fourthly, we secured introspective data during the experiment, but these will not be considered separately, but at such points where they will be of significance.

It will perhaps be simpler first to give the massed results irrespective of individual observers. After each general tendency thus pointed out, we may consider whether and why certain individuals deviated from the tendencies shown by the group. We should expect, from previous experiments in this field, that *Fk.* would be weakest in the first experiment with full reduction and strongest in the last experiment with good *Feldgliederung*. This was emphatically the result for all standard grays with the exception of the brightest one (265 degrees white). For this particular gray, *Fk.* was best in the first experiment, but was judged as far too white in the other three experiments. Reëxamination of the conditions of the experiment shows that the whiteness of this gray was exaggerated through contrast with the large sector of the background exposed in Experiment III, and with the

average dark field of the 29 grays surrounding the standard gray in Experiment IV. The remaining standard grays, however, show invariably a stronger *Fk.* in the fourth as compared with the first experiment. The average error made in judging all five standards (including the atypical gray of 365 degrees white) was 103.84 degrees in the first experiment and only 6.32 degrees in the fourth experiment. This difference is vast. The corresponding average deviations for all observers and for all standards is 35.67 degrees in the first, and only 7.43 degrees in the fourth experiment.

We add a few remarks regarding individual differences. Each one of the three observers serving in both Experiments I and IV manifested the group tendency just discussed; that is, showed a vastly better *Fk.* for the fourth than in the first experiment, and one of them (W., the least trained in laboratory observation!) showed this for the white of 365 degrees also.

We turn now to the problem of the effect of the varying illumination, a condition under which *Fk.* has never before been studied. The table below gives the significant data:

AVERAGE COMPARISON GRAYS WITH DEVIATIONS, CHANGING *versus* CONSTANT ILLUMINATION

Light	265		104		63		42		17	
	Av. G.	Av. Dev.	Av. G.	Av. Dev.	Av. G.	Av. Dev.	Av. G.	Av. Dev.	Av.G.	Av. Dev.
I. Cha.	272.7	41.9	250.1	41.3	215.9	39.7	188.1	37.6	147.5	29.4
Con.	244.8	25.2	217.5	34.9	161.6	31.3	158.0	39.2	126.3	36.3
II. Cha.	295.4	36.0			158.5	49.4			95.8	40.1
Con.	216.4	48.8			141.0	33.2			110.7	17.3
III. Cha.	304.9	14.6	140.3	22.5	65.1	9.3	45.9	6.0	16.3	5.4
Con.	290.9	13.8	122.4	14.4	63.0	7.5	42.2	4.7	19.6	2.0
IV. Cha.	291.6	31.3	87.7	2.8	61.5	9.2	38.0	5.2	15.4	1.9
Con.	264.0	00.0	81.9	10.8	63.7	7.5	36.1	1.8	20.2	3.8

The Roman numerals to the extreme left of the table refer to the four experiments. For each experiment, a separate average was made for changing illumination (Cha.) and another average for constant illumination (Con.). The

column headed Av. G. (Average gray) gives the averages of the comparison grays selected by all of the observers for each one of the standard grays. The column headed Av. Dev. (average deviation) gives the average deviations of the observers' choices from their own average comparison grays.

An examination of the table shows that *Fk.* cannot be said to exist at all for the first experiment. In the second experiment *Fk.* is evident to a slight extent, and becomes quite pronounced in the third and fourth experiments. The average error for changing illumination, for all standards taken together is 42.10 degrees for constant illumination, while for changing illumination it is 51.15 degrees. This shows that *Fk.* is somewhat better for constant than for changing illumination, but the difference is vastly less than one would expect recalling that the illumination changes cover a range of 1 : 5 in the short period of one second, and that the O's have sometimes to make their judgments under conditions of extreme reduction of illumination. The corresponding average deviations are 18.46 degrees for constant illumination as against an average error of 23.53 degrees for changing illumination, again a small advantage for constant illumination. The claim that *Fk.* obtains despite the presence of extensive and swift changes of illumination, therefore, stands. The bearing of the facts on current theories will be taken up in Section IV.

We confine our remarks regarding individual differences to the three observers taking part in all four experiments. When the results for all five standard grays are averaged, it is shown that each observer exhibits this superiority for constant as compared with changing illumination. Observer Ke. makes an average error of 45.24 degrees for changing and an error of 32.32 degrees for constant illumination. The corresponding figures for Br. are 69.42 for changing and 59.27 for constant illumination; while for W. the error for changing illumination in 89.31, and for constant illumination 69.01 degrees.

The conclusion from the above results that *Fk.* obtains during changing illumination, has to meet one important

criticism. It may be that the observers are influenced only by the terminal grades of the changing light, that is, for instance, that when a gray is seen with illumination going up, the O judges it at the bright point, thus nullifying the influence of the changing light as a whole. That this does not occur is shown by the following computations. If we make four averages, one for each condition of illumination, pooling the results of all four experiments, we get the following results:

1. Average error, illumination going up..... = 51.71
2. Average error, illumination going down..... = 47.94
3. Average error, illumination at bright extreme..... = 60.35
4. Average error, illumination at dark extreme..... = 33.23

All of the above errors are plus errors, meaning that the whiteness values of the standards were always overestimated. This error is greatest and least for the two conditions of constant illumination, ranging from 33.23 to 60.35 degrees. But the errors for changing illumination do not approach these extremes but lie together near a point two-thirds the distance from the dark illumination, one-third the distance from the bright illumination. *Fk.* is not perfect for any condition of illumination, being influenced by each illumination. The important point here is that changing illumination has an influence lying between the dark and the bright illuminations.

We now turn to the general results regarding the average 'subjective variations,' namely, the average range of comparison colors given by the O's in locating the standard colors. The results here are what one would expect from the other results just given. If we compare the first with the fourth experiment, we find again that the subjective deviations for the first experiment, with complete reduction, is much larger (average = 43.26 degrees) than it is in the fourth experiment (average = 6.09 degrees). Again, if we average the subjective variations separately for changing as against constant illumination we find the variations less for constant (19.40 degrees) as compared with changing illumination (29.96 degrees). Some observers tended, when uncertain where the standard should be located, to give the mid-point of a range of comparison grays. The subjective variations were naturally larger for constant light at the dark point. In the first

experiment the ability to see true surface color was very marked, hence the absence of *Fk.* in this experiment. The observers preferred the conditions with the illumination at the bright point or going towards it. Opinions differed regarding which one of these two was the best. Observers Br., Be. and W. preferred the bright constant light while Ka. and Ke. tended to favor the illumination changing from dark to light.

Special interest attaches to the results of Ke. because he is night blind. Despite this condition, his results are, in general, the same as for the other observers. All the averages calculated above include his results; and if his results are left out the major tendencies remain the same. But closer analysis of the results from Ke. shows special features. For him, constancy failed completely in the first experiment, as it did for the other observers. More than the other O's, however, Ke. tended to see all standard grays as alike with the light at the dark extreme. An actual range of brightness from 265 to 17 degrees is seen to vary only from 68 to 32 degrees. An observer with night-blindness would be incapacitated in these experiments because the process of adaptation in night blindness is too slow to keep pace with the changing illumination. But, in Experiments III and IV, with the introduction of *Gliederung*, Ke. judges with as much success as the other observers even with the light at the dark point. This indicates that the adverse effects on visual perception of night-blindness can be compensated or corrected by the constancy principle. *Fk.* appears when *Gliederung* appears, but this does not show that *Gliederung* is the chief condition of its appearance.

IV. Experimental Results and Theories

Color constancy, as a phenomenon, is a striking one, and no refinements of apparatus are required to demonstrate its existence. Impressive as a phenomenon, it is still a baffling problem to the theorist. That the factors conditioning *Fk.* are obscure and difficult to bring to light is manifest from a study of the various attempts made to find a consistent explanation for it.[7]

[7] See the critical review of theories given by D. Katz in his new edition, Die Aufbau der Farbwelt, 1930, pp. 430–475.

Four major accounts of constancy may be mentioned, the last two in more detail since they concern the rôle played by illumination in the constancy phenomenon. Jaensch [8] has given evidence which appear to show that *Fk.* cannot be referred to simultaneous contrast and other types of adaptation. Yet, he finds it necessary to suppose that constancy and contrast have a common root. If one is to be explained in terms of higher processes then so must the other. Jaensch finds laws, which he calls parallel laws, which he claims are valid for both contrast and constancy. In our experiments, contrast more often than not produced effects in the same direction as constancy. Jaensch's view has met with factual criticisms, chiefly from G. E. Müller [9] and from experiments from the Rostock laboratory.[10]

Thouless is one of the very few non-German authors who has made contributions to the subject of constancy. He does not confine himself to color constancy and to the attempt to explain it. Instead, on the basis of his own experiments as well as the results of others he attempts to extend the range of phenomena to which the principle of constancy applies.[11] In all cases where constancy appears, whether for colors, for grays, for forms, positions or for size, constancy constitutes a 'regression to the real object.' This means, that constancy produces a phenomenal appearance which lies midway between present conditions of stimulation, and the real nature of the object. Thus, an inclined coin is seen as an ellipse, but the degree of ellipticity is not so great as that required by the laws of perspective. The appearance of the coin regresses to its 'real' form, that of a circle. As thus conceived, Thouless applies the constancy principle to well-known phenomena of psychology, thus presenting them in a new light. Most significantly of all, constancy is presented as a factor that accounts for the differences between Fechner's and Merkel's formulations of Weber's law.

[8] E. R. Jaensch and E. A. Müller, Ueber die Wahrnehmung farbloser Helligkeiten und den Helligkeitscontrast, *Zeits. f. Psychol.*, 1920, **83**, 266–341. See also Chs. 1, 2 and 3 of Ueber Grundfragen der Farbenpsychologie, Leipzig, 1930.

[9] G. E. Müller, *Zeits. f. Psychol.*, 1923, **93**, S. 1–16.

[10] T. Haack, Kontrast und Transformation, *Zeits. f. Psychol.*, 1929, **112**, 93–138.

[11] R. H. Thouless, Phenomenal regression to the real object, I and II, *Brit. J. of Psychol.*, 1931, **21**, 339–359; and **22**, 1–30.

What rôle does illumination play in constancy? This question engaged the attention of early authorities.[12] With modern investigators, the rôle played by illumination is still central, and Bühler's theory especially assigns to it a place of importance in explaining constancy.[13] Bühler supposes that atmospheric illumination (*Luftlicht*) which is reflected from dust and gas particles in the air is the main external condition to be considered in explaining *Fk*. Bühler apparently explains the phenomenon as follows: The eye receives two types of light waves: (1) those reflected from the object seen, and (2) light reflected from the space about the object, the so-called *Luftlicht*. We have here the factors necessary for seeing the object in constant color or brightness, since a correction can be made on the basis of the perceived atmospheric illumination. That is, the piece of white paper, for instance, can still be seen as white in twilight, since this twilight is separately perceived, and an allowance can be made for it by some mechanism, neurological or mental. The difficulties of this view, on both experimental and logical grounds, have been set forth by professor Katz.[14]

Illumination, according to Katz, does play a rôle in *Fk*. But in a way quite different from that supposed by Bühler. Like Gelb, Katz points out the distinction between *illumination and the object illuminated*. The structurization of a visual field (*Gesichtsfeldgliederung*) is not the chief clew by means of which we judge illumination. This is proved by the fact that as illumination becomes very high or very low it causes an obliteration of structure and contour in the field, but illumination nevertheless appears to increase or decrease. Katz finds it necessary to call attention to a new factor, that of the total impressiveness (*Gesamteindringlichkeit*, hereafter symbolized as *Ge*.) of the visual field. By *Ge*. is meant the strength with which the colors or grays from external objects or papers invade consciousness. *Ge*. is totally different from

[12] H. v. Helmholtz, Handbuch der Physiologischen Optik, 2 Aufl, 1896, S. 110.

[13] K. Bühler, Handbuch der Psychologie, I, Teil, Jena, 1922.

[14] D. Katz, Neue Beiträge zu den Erscheinungsweisen der Farben. Luftlicht und Beluchtungseindruck, *Zeits. f. Psychol.*, 1924, **95**, i Abteil, S. 129–136; and Der Aufbau der Farbwelt, 1930, S. 430–475.

brightness or color as such. One may have a color or gray of a given brightness and saturation, and vary its impressiveness as an independent factor. This is accomplished by varying the illumination. If the visual field is uniformly lighted the grade of *Ge.* is easily perceived. But the *Ge.* of a complex field can be made apparent by viewing it through the aperture of an episcotister. When the aperture of the episcotister is nearly closed, *Ge.* approaches zero, the level of the visual gray. As the aperture is widened, some objects acquire a higher *Ge.* than the visual gray because they are brighter; but a shade may get a higher *Ge.* also because it is darker than the visual gray. This makes it clear that *Ge.* is not identical with brightness of the objective color or with gray, or with illumination. *Ge.* has to do with phenomenal impressiveness and may attach to a dark as well as to a light gray.

Ge. is correlated with illumination and also with the color and brightness of the object seen. But the influence of color and brightness on *Ge.* are small because their range in nature is small. The brightness differences of objects in the usual visual field are far less than we would suppose. The ratio of pure white paper to *Tuchschwarz* is about 1 to 60. The range of brightness in nature is far less than this. On the other hand, the illumination of objects shows a vast range, expressed by the ratio 1 to 3600, this representing the differences between a dark night and noonday illumination.[15]

The experiments of Krüger show that it is possible to make judgments of the condition of illumination.[16] But the illumination of the air could not, as Bühler supposes, supply the cue for the correct apprehension of color or shade. The air does have an illumination but this illumination for distances within which color constancy appears with strongest effects, is below the threshold of perception, as proved in experiments by Professor Katz. The illumination of space is in fact determined chiefly by the light reflected from objects. Now arises a difficulty for the theory of Bühler which is logical in

[15] *Cf.* D. Katz, *op. cit.*, S. 461 ff.

[16] H. Krüger, Uber die Unterscheidsempfindlichkeit für Beleuchtungseindrücke, *Zeits. f. Psychol.*, 1925, **96**, 1 Abteil, 58–67.

nature, the same difficulty which Hering urged against Helmholtz.[17] If the brightness of the object determines the impression of illumination, it is a circular argument to maintain that we arrive at the true brightness of the object by setting aside the effects of illumination.

Bühler's theory is then between the horns of a dilemma, the one horn consisting of the experimental fact that *Luftlicht* is below the threshold of perception for distances within which constancy appears, the other horn of the dilemma is a logical *impasse*. Katz seeks to solve the difficulty by use of the concept of *Ge*. *Ge*., in Katz's theory, plays the rôle of a corrective agency, just like the rôle which Bühler sought to attribute to the *Luftlicht*. The difference is that the *Luftlicht* is not verifiable as a significant factor in the phenomena in question, whereas *Ge*. is demonstrable with the episcotister, as described. Katz maintains that two identical papers, one seen in shadow and the other under good illumination, may look quite alike so far as color, grayness or brightness is concerned, but the one in shadow appears as less *Eindringlich*. This difference is the basis for the correction which enables us to see a paper in a constant or nearly constant shade, despite changes of illumination. Physically, illumination determines the degree of *Ge*., but psychologically, the degree of *Ge*. is the cue for illumination.

The genetic point of view enables us to see a second major difference between such views as those of Helmholtz, v. Kries and Bühler on the one hand, and those of Gelb and Katz on the other. The first mentioned views are either explicitly or implicitly empirical. It is supposed or implied that the object is seen in its true color or shade because we learn on the basis of experience that the reighning illumination is taken into account in judging the color. Since the first edition of his work, Katz emphasizes the rôle of experience less and less and the rôle of native factors more and more.[18] The association of a given illumination with a given appearance of a thing is not piece-meal association, not the mere association by

[17] E. Hering, Zur Lehre vom Lichtsinn, Teil I, Kap. 12, Leipzig, 1905 und 1907, S. 20.

[18] For details, see Der Aufbau der Farbwelt, S. 469–474.

repetition which unites nonsense syllables. We have to do here with systematic or chain association. Every gray belongs in its proper place in the series from black to white, for structural reasons. A dark gray near the white end of the series will never appear to *belong* there, no matter how often it is seen in this position. Just so, the facts of color constancy are bound up in series. A given shade of white in one illumination has a corresponding shade in every other illumination, but not all of the other illuminations need to be seen for constancy to manifest itself in them. That color constancy does not improve with experience is already indicated in the results of Burzlaff, who shows that previous results indicating an improvement of *Fk.* with increasing age are the consequence of special procedures used. Other methods show that *Fk.* is already highly developed in young children.[19] Thouless also shows that the observer's previous knowledge of the object is not necessary for constancy to appear.[20] Constancy, therefore, rests on cues supplied in the reighning conditions of perception.

It must be said, however, that no one, so far as the author knows, has tested the effects of practice carried out for a long period of time. In any case, though not resting on individual experience there is no ground for denying that constancy, as a corrective for perception, may have been acquired in the experience of the race. True, Köhler finds constancy in the perception of chimpanzees and chickens, an observation verified by Katz and Revesz. V. Frisch has found a form of constancy in the perception of fishes.[21] But even an animal low in the evolutionary scale has had a long history as a species.

As regards our experiments, there are occasions in daily life in which we experience large, sudden, and perhaps even gradual changes of illumination.[22] This occurs, for instance, when a sheet of paper is turned so that the light falls on it from

[19] W. Burzlaff, *op. cit.*, pp. 177–235.

[20] R. H. Thouless, Phenomenal regression to the real object, I and II, *Brit. J. of Psychol.*, 1931, **21**, 339–359, and **22**, 1–30.

[21] Facts reported by Burzlaff, *op. cit.*, S. 209.

[22] This matter is discussed by Katz, *op. cit.*, Section 73.

a new angle, so that it passes from brilliant illumination to shadow. It occurs when a light is turned on, or when an object is carried from one room to another quite differently lighted. But all such natural events have this in common, that the object seen begins with a constant state of illumination, and ends in another constant state. The observer can always wait and judge it after a change has taken place, and does not need to judge the object while illumination is changing. Our experimental conditions were however unique in this respect, for the observer never had the opportunity to see the object except when its illumination was in the process of change. But since *Fk.* nevertheless exists under these conditions, provided there is structurization of the field, the conclusion is suggested that color constancy arises from some original connection between grades of illumination and the phenomenal appearances which colors and brightnesses will have in them. There is apparently something which tells the eye that illumination changes, not the brightness of the object seen. What is this sign or cue? The gray in twilight looks like an equal gray in daylight so far as its quality is concerned, but it is less impressive in twilight. Physically, the changing illumination causes the changing impressiveness, but psychologically, the changing impressiveness is the sign (*Merkmal*) that illumination has changed.

(Manuscript received August 3, 1932)

AN ATTEMPT TO CONDITION BREATHING AND EYELID RESPONSES TO A SUBLIMINAL ELECTRIC STIMULUS

BY HULSEY CASON AND NAOMI KATCHER

University of Wisconsin

Although it has been demonstrated that subliminal stimuli can influence bodily activities, it has not yet been shown that a connection can be formed between a subliminal stimulus and a conscious or unconscious response by the process of learning. Before a conscious activity comes into existence, the energy that is produced or liberated in the body must be of a certain required strength, but nervous energy frequently leads to some bodily effect even though its strength is not sufficient to produce a conscious activity. It seems reasonable to believe that energy of this subliminal strength can be connected with some response by the learning process, and if this could be demonstrated the theoretical implications would be important. The conscious factor would be eliminated on the stimulus side, and the subject would learn something without knowing what the stimulus was with which the response was connected. Such a result would give a suggestion in regard to the development of various physiological activities before and after birth, and the finding might afford an explanation of several functional disorders and dissociated conditions. If learning to a subliminal stimulus could be demonstrated it would be plausible to believe that one could become conditioned to several unconscious stimuli from the rhythmical activities of the body, as well as to various subliminal stimuli such as sounds and odors in the everyday environment.

We have made a serious attempt to establish learning of this kind, but our attempt has not been successful and our results do not show that a response can or cannot be connected with a subliminal stimulus by the learning process. The experiment is described, however, and several suggestions are

made in the hope and also with the expectation that some other experimenter will succeed where we have failed.

Since only a small effect is obtained when many subliminal stimuli act on the exteroceptors, it appeared that the most that could be hoped for was to produce a small *change* in some response that was influenced by a large number of factors and which was going on all of the time. We have attempted to condition breathing and eyelid responses to a subliminal electric stimulus, and the electric stimulus had a strength approximately one-third below the threshold value.

The general method used and the 4 different procedures employed were as follows. When a light was turned on, S voluntarily took a deep breath, and while he was breathing in this way the subliminal electric shock was on. After the deep breathing and the subliminal shock had been present simultaneously a number of times, a test was made to determine whether or not the subliminal shock, without the light signal, would cause an increase in the amount of breathing. Two opposite training procedures were used with the breathing response, and two also with the eyelid response. The 4 training procedures were (1) training toward an *increase* in the amount of *breathing*, (2) training toward a *decrease* in the amount of *breathing*, (3) training toward an *increase* in the frequency of the *eyelid* response, and (4) training toward a *decrease* in the frequency of the *eyelid* response. In the second training procedure the training consisted in having S hold his breath and keep the voluntary muscles of his trunk in as stationary a position as possible while the light signal was present for 5 sec. and while the subliminal shock was on. In the third training procedure the training consisted in having S wink voluntarily as rapidly as possible while the light signal was present for 5 sec. and while the subliminal shock was on. In the fourth training procedure the training consisted in having S attempt to inhibit the winking response while the light signal was present for 5 sec. and while the subliminal shock was on. With the data that were obtained it is possible to compare the effect of training toward an increase in the amount of breathing with the effect of training toward a

decrease in the amount of breathing, and also possible to compare the effect of training toward an increase and training toward a decrease in the frequency of the eyelid response.

The experiment was planned by Cason, and a preliminary study was made by Miss Naomi Katcher. Ten S's were used in her study, the training was toward an increase in the amount of breathing, and the results at first seemed positive. Cason later made some improvements in the procedure and apparatus and carried out a second preliminary experiment with 11 S's, the training here also being toward an increase in the amount of breathing. The results of this second preliminary experiment were quite irregular, but they seemed at first to be slightly positive. After the second preliminary experiment, we thought that there were still a number of defects in the method used, and we were not certain that the subliminal stimulus had been adequately controlled. The first-named author made a new start with an improved procedure, and the present paper is concerned only with the results which he later obtained with 24 S's.

Apparatus.—E and S were in different rooms some 40 ft. apart, and the rooms were separated by 3 doors and one wall. No perceptible sound reached the S from the apparatus in E's room. A cable of wires connected the apparatus in the 2 rooms. The signals from E to S were a light and a telegraph relay, and the signal from S to E was a telegraph relay. S was seated in a very comfortable chair.

The source of current for the *subliminal electric shock* was the electric light current of 110 volts and 60 cycles. This current was reduced by an alternating current electro therapeutic regulator, and it was in circuit with a rheostat from which shunted current passed to the S's arm. A pointer on the slider of the rheostat indicated on a scale the strength of the shock being used, and the amperage was directly proportional to the units of the scale.

The *electrodes* connecting the S with the apparatus which regulated the current in E's room were 2 sponges, each 2″ × 1¼″ × ½″, which were placed some 6″ apart on the upper surface of S's left arm. The pores of the sponges were first filled with a ½ percent salt solution and then about one-half of the solution was pressed out. Rubber sheeting was wrapped around the S's arm and around the electrodes, and the sheeting was held in place by several loose-fitting, thin rubber bands. S's left arm rested comfortably on a large board which was fastened to the arms of the chair, and S was instructed to keep this arm in as relaxed a position as possible and to refrain from touching it with his right hand during the experiment. If a numb sensation appeared in S's left hand or arm, he was asked to signal E, who adjusted the thin rubber bands so that the sheeting and the thin bands would not interfere with circulation any more than was necessary.

The *device used to measure breathing* was a belt apparatus which fitted closely around the S's body in the general region of the epigastrium.[1] Whenever the total length of the belt changed, a mechanism in the belt made a series of electric contacts, and the number of contacts made was directly proportional to the change in the circumference of that part of S's body located just beneath the belt. Separate measures were not obtained of inspiration and expiration, but accurate records were secured of the change in the circumference of S's body for given intervals of time. The breathing apparatus was in circuit with a fast marker in E's room, and the stylus of this marker recorded the electric contacts made by the mechanism in the belt apparatus on the smoked paper of a long paper kymograph. A record of time was made by a Jacquet graphic chronometer. The third line on the smoked paper was made by a signal marker which indicated when the subliminal shock was on and off.

The *apparatus used to record the time and the number of eyelid responses* was similar to that employed in an earlier study.[2] One end of a thin aluminum lever was attached to the S's left eyelid, and the lever was supported just to the left of his left eye by a pair of 'spectacles' which could be properly fitted to individual S's and worn comfortably for an indefinite period of time. The inner end of the lever was attached to S's eyelid just above the free margin by a piece of sticking plaster. The lever could slide to the right and left in the fulcrum, and it was balanced and adjusted in such a way that, after a few minutes of adaptation, S was scarcely conscious that it was attached to and was moving with his eyelid. A piece of platinum wire was attached to the outer end of the lever, and when S winked this wire made an electric contact with another piece of platinum wire which was fixed in a favorable position.

For half of the S's the fast marker was in circuit with the eyelid apparatus instead of the breathing apparatus, and a record was obtained of the time of each eyelid response and also the number of eyelid responses which occurred during given intervals of time. The eyelid apparatus which we used is quite simple and has a number of practical advantages over other more complicated and more frequently used methods.

By means of suitable electric connections, etc., the subliminal shock could be turned on and off for given intervals of time while a continuous record was being made of the amount of breathing or the number of eyelid responses; and the light signal and the subliminal shock could be turned on and off simultaneously.

The records that were made on smoked paper were (1) a record of the amount of the breathing or the number of eyelid responses during given intervals of time, (2) when the subliminal shock was on and when it was off, and (3) the time in seconds. The top response line and the bottom time line were .3″ apart, and some 120 ft. of record were made with each S.

Subjects.—Twenty four graduate and undergraduate students acted as S's. Both sexes were used with the eyelid response, but only men were employed in the experiments on breathing. The majority of the S's did not know the purpose of the experiment, but this knowledge on the part of some of them did not prejudice the results in any way.

Procedure.—Cason acted as E for all of the S's, and the individual experiment lasted between 3 and 4 hrs. It con-

[1] This apparatus is described in H. Cason and E. B. Cason, Affectivity in relation to breathing and gross bodily movement, *J. Gen. Psychol.*, 1933, *9*, 130–156.

[2] H. Cason, The conditioned eyelid reaction, *J. Exper. Psychol.*, 1922, **5**, 153–196.

sisted of the following 4 parts. (1) At the beginning of the experiment the natural effect of the subliminal shock on the response was measured, and this was the *first control test* (CT1). (2) The *conditioned response training* consisted in repeatedly turning on the subliminal shock while one of the 4 responses was taking place. A kymograph record of the response was not made during the conditioned response training, but E noted at all times whether or not S was carrying out the instructions. The *conditioned response test* (CRT) consisted in measuring the effect of the subliminal shock on the response after several repetitions of the conditioned response training combination had occurred, and the procedure in CRT was the same as in CT1. Several repetitions of the conditioned response training combination and a CRT were alternated with each other a number of times. (3) After a number of conditioned response training combinations and CRT's, there was a period of '*adaptation*' in which the subliminal shock remained on continuously for 15 min. (4) At the end of the experiment, the effect of the subliminal shock on the response was measured once more, and the procedure in this *second control test* (CT2) was the same as in CT1 and in the CRT's.

The routine procedure in the individual experiment with each S was as follows.

1. The apparatus was put on the S and adjusted, S was seated, and the large board was attached to the arms of the chair. The electrodes and rubber sheeting were placed on S's left arm, and the tension of the thin rubber bands was adjusted.

2. The general facts about supraliminal and subliminal (electric) stimuli were explained to S, and the method which was to be used in determining his threshold was described. S was instructed to press the signal key promptly whenever he felt a shock come on or whenever he thought or suspected that he felt a shock, however weak the shock might seem to be. S's signal key operated a telegraph relay in E's room. The point that was most emphasized in the instructions to the S was that he must press the signal key promptly whenever he thought he felt a shock of any kind come on.

3. S's threshold for the electric stimulus was determined in the following manner. E turned on the current and kept it on for 2 or 3 sec. and noted when an if S signalled that he felt the shock. Sometimes E signalled S to pay special attention to his left arm and to signal promptly when and if any shock came on. If S signalled that the shock was on when it was off, E signalled S that he was wrong and that what he felt in his left arm was not the shock. S was tested with currents of various strengths and with the shock sometimes on and sometimes off until his approximate threshold had been determined. E then questioned S on the way the shock felt and where he felt it. In some cases it was necessary to make a further adjustment of the thin rubber bands around S's arm in order to decrease the interference with circulation as much as possible. S was then practised for some 15 min. in judging when a weak electric shock came on, and this practise generally resulted in a further lowering of the threshold. When S's threshold seemed to be relatively stable, the amperage was reduced one-third below the threshold value, and a shock of this reduced strength was used as the subliminal electric stimulus. The threshold was retested every 30 min. during the experiment, and the amperage of the subliminal stimulus was kept approximately one-third below the threshold value. The threshold decreased during the experiment with the majority of the S's, and it was necessary to take considerable pains to keep the record from being ruined.

4. Part I. S was told to remain in a relaxed but natural position for 15 min., and during this period he was not allowed to smoke or read. When the eyelid response was being used, S held his eyes open and looked at a horizontal level and straight to the front. The first control test (CT1) was carried out under these conditions. While a continuous record of the response was being made, the subliminal shock was repeatedly turned on and off at intervals of 5 sec. A record was made of the amount of the response, the intervals during which the subliminal shock was on and off, and the time in sec. The influence of the subliminal shock on the breathing or eyelid response could be readily determined from the record.

5. Part II. After CT1, S was given instructions for one of the 4 training procedures. A conditioned response training procedure and the conditioned response test were alternated with each other a number of times. In each repetition of each of the training combinations, the subliminal shock was on while S was responding voluntarily for 5 sec. There were some 15 repetitions of the voluntary response and the subliminal shock, and after each repetition of the combination there was an idle period averaging about 20 sec. The S was then signalled to sit in a relaxed and natural position and a conditioned response test (CRT) was made while he was resting. Measures were made of the amount of the response while the subliminal shock was alternately on and off, and the procedure was the same as in CT1. In a single CRT, there were some 6 measures of the response with the shock-on and 6 with the shock-off. After each CRT, S was signalled that his occupation was changing again, and the conditioned response training procedure was continued. There were some 15 conditioned response training periods and 15 CRT's with each S.

6. Part III. S was asked to rest, and the subliminal shock was kept on continuously for 15 min. The purpose of this procedure was to adapt or fatigue any connection that may have been formed.

7. Part IV. The second control test (CT2) was made at the end of the experiment, and the procedure was the same as in CT1 and in the CRT's.

In CT1, in the conditioned response training period, in CRT, and in CT2, 5-sec. intervals were used with 3 S's for each of the 4 procedures, and 10-sec. intervals were used with 3 other S's for each of the 4 procedures. The routine of the experiment with a single S has been described above for the 5-sec. intervals, and corresponding changes were made in the procedure when 10-sec. intervals were used. The general conditions of the experiment were well controlled, and the results can be accepted at their face value.

Results.—In the record obtained with each S, a count was made of the number of breathing contacts or the number of

eyelid responses for each of the following 6 occasions: CT1 Shock On, CT1 Shock Off, CRT's Shock On, CRT's Shock Off, CT2 Shock On, and CT2 Shock Off; and an accurate measure was secured of the influence of the subliminal shock on the response in CT1, CRT's, and CT2. We have calculated the N., Av., and Mdn. for each of these 6 distributions and for each of the 24 S's.

The results of the individual S's do not show any consistent evidence of conditioning in any one of the 4 procedures. There is also no reliable difference between the results for the 5- and 10-sec. intervals.

The combined results of the 6 S's in each of the 4 procedures are given in Tables 1, 2, 3, and 4, and in each table the results obtained with the 5- and 10-sec. intervals have been combined. In Tables 1 and 2 on the breathing response, the highest 10 percent of the scores for each S have been given a value equal to the 90th percentile; and decreasing the highest scores in this way partially discounted the disturbing effects of sighing, yawning, and coughing on the breathing scores. The range in the breathing scores is from 0 to 15. In Tables 3 and 4 on the eyelid response, the highest score allowed was 7, and this decreased the value of about 3 percent of the eyelid scores.

In the 4 tables the average and median scores for CT1 show that before training the subliminal shock had little, if any, natural effect on the breathing and eyelid responses. In Table 1, if the training toward an increase in the amount of

TABLE 1

Breathing RESPONSE SCORES IN WHICH THE TRAINING WAS TOWARD AN *Increase* IN THE AMOUNT OF BREATHING. COMBINED RESULTS OF SIX SUBJECTS.

	CT1		CRT's		CT2	
	Shock On	Shock Off	Shock On	Shock Off	Shock On	Shock Off
N	308	310	312	369	295	297
Av	8.51	7.98	5.75	6.25	6.82	6.97
Mdn	8.76	8.42	5.50	6.00	6.45	6.70
Ratio of Shock On to Shock Off	Av. 1.07		.92		.98	
	Mdn. 1.04		.92		.96	

TABLE 2

Breathing RESPONSE SCORES IN WHICH THE TRAINING WAS TOWARD A *Decrease* IN THE AMOUNT OF BREATHING. COMBINED RESULTS OF SIX SUBJECTS.

	CT1		CRT's		CT2	
	Shock On	Shock Off	Shock On	Shock Off	Shock On	Shock Off
N	368	371	395	416	341	344
Av	6.59	6.23	6.05	6.00	4.94	5.06
Mdn	6.50	6.15	5.35	5.29	4.78	4.76
Ratio of Shock On to Shock Off	Av 1.06		1.01		.98	
	Mdn 1.06		1.01		1.00	

TABLE 3

Eyelid RESPONSE SCORES IN WHICH THE TRAINING WAS TOWARD AN *Increase* IN THE FREQUENCY OF THE EYELID RESPONSE. COMBINED RESULTS OF SIX SUBJECTS.

	CT1		CRT's		CT2	
	Shock On	Shock Off	Shock On	Shock Off	Shock On	Shock Off
N	457	461	500	500	425	428
Av	2.73	2.69	3.02	3.08	3.26	3.20
Mdn	2.30	2.26	2.89	2.81	2.95	2.80
Ratio of Shock On to Shock Off	Av 1.01		.98		1.02	
	Mdn 1.02		1.03		1.05	

TABLE 4

Eyelid RESPONSE SCORES IN WHICH THE TRAINING WAS TOWARDS A *Decrease* IN THE FREQUENCY OF THE EYELID RESPONSE. COMBINED RESULTS OF SIX SUBJECTS.

	CT1		CRT's		CT2	
	Shock On	Shock Off	Shock On	Shock Off	Shock On	Shock Off
N	546	549	603	603	771	767
Av	2.03	2.08	2.60	2.68	2.26	2.30
Mdn	1.70	1.68	2.15	2.28	1.74	1.88
Ratio of Shock On to Shock Off	Av .98		.97		.98	
	Mdn 1.01		.94		.93	

breathing had had any positive effect, the central tendencies of the breathing scores for shock-on would have been greater than the central tendencies of the scores for shock-off. How-

ever, the ratios of shock-on to shock-off are .92 for both the average and the median. It may seem from this result that the training produced an effect in the opposite direction, but in Table 3 the corresponding ratios for the eyelid response are .98 and 1.03.

In Table 2, if the training toward a decrease in the amount of breathing had had any positive effect, the central tendencies of the breathing scores for shock-on would have been smaller than the central tendencies of the scores for shock-off. However, the ratios of shock on to shock off are 1.01 for both the average and the median. It may seem from this result that the training produced a very slight effect in the opposite direction, but in Table 4 the corresponding ratios for the eyelid response are .97 and .94.

The distributions of the breathing scores are fairly normal, but the eyelid scores are skewed toward the higher values. There is no reliable difference between the variability of the scores for shock on and shock off. No precise calculations have been or should be made of the reliability of the measures, but several good comparisons can be made between the results obtained with the 4 different procedures. The results obtained with the different procedures show quite clearly that we did not succeed in conditioning breathing or eyelid responses to the subliminal electric shock.

The above results are based on the total number of breathing and the total number of eyelid responses in each of the 5- and 10-sec. intervals, and the question remains whether or not any conditioning was present in the distribution of the breathing and eyelid scores within each interval under the different conditions. Miss Miriam Long has kindly made the necessary calculations for each sec. in each interval, under every condition, and for both individuals and groups. There was no difference in the distribution of the breathing or eyelid scores for shock on or for shock off for the three occasions, CT1, CRT's, and CT2. The individual and combined results of the experiment are therefore negative when they are based on the total scores for each interval and also when they are based on the distribution of the scores within each interval.

Suggestions on Procedure.—Although the results that we obtained do not show that a response can or cannot be conditioned to a subliminal stimulus, the study may be of some value in suggesting several modes of attack on this important problem. In a further investigation it would not be necessary to spend as much time as we did on control tests. Our results show what can be expected of the natural influence of the kind of subliminal stimulus that we used on the breathing and eyelid responses. After the experience of carrying out the experiment, it now seems to the writer that the following procedures and experiments have a greater probability of giving positive results than the method which we employed.

1. A much larger number of repetitions of the conditioned response training stimuli could be used. The voluntary response and the subliminal shock could be repeated 100 times, then 10 tests could be made with shock-on and shock-off, and so on, alternately, with necessary rest-periods, until there had been 1000 or more repetitions of the conditioned response training combination and 100 or more tests with shock-on and shock-off.

2. Instead of having S respond voluntarily in the training series when a light is turned on, he could respond voluntarily when he feels a supraliminal electric shock, and the subliminal shock could be omitted in the training series. This kind of training could be repeated 1000 or more times, and S's ability to indicate when a faint shock comes on could be markedly improved. After considerable practise in responding voluntarily to a supraliminal shock, S could be tested with a shock that was perhaps only 5 percent below the threshold. By means of this procedure it could be determined whether improvement in responding to a faint but noticeable stimulus in one sense modality can affect one's ability to respond to a just subliminal stimulus in the same modality.

3. A subliminal auditory stimulus could be used instead of the subliminal electric stimulus if the external conditions of the experiment were well controlled.

4. One could attempt to condition the pupillary light

reflex to a subliminal stimulus, and if this were successful it would seem that the result would be of considerable importance.

5. The last suggestion that we care to make is concerned with the judgment process, and the experiment could be carried out somewhat as follows. The S could be given 3 signals separated by 4-sec. intervals. The first signal could be a ready signal, and between the second and third signals the intensity of a light could be increased or decreased a noticeable amount. After the third signal, S could report whether the light had just increased or decreased. The change in the intensity of the light could be quite small but not so small as to result in any errors of judgment. Between the second and third signals, a subliminal electric shock could always be on while the intensity of the light was increasing, and always off while the intensity of the light was decreasing. The direction of the training could be reversed at different times and with different S's. In the test series after training, S could be told that between the second and third signals the change in the intensity of the light would be so small that he probably would not be able to notice it, but that he must report either 'increase' or 'decrease' for each trial. Using the procedure with the 3 signals as in the training procedure, the intensity of the light could be kept constant, and between the second and third signals the subliminal shock could be turned on and could remain off in an irregular order. The subliminal shock could be turned on and could remain off an equal number of times, and S could also be required to report 'increase' and 'decrease' the same number of times in the end. Appropriate mechanical devices could be used to give the 3 signals, to turn the light on and off, and to turn the subliminal shock on and off. Sound or some other stimulus might give better results than light in this experiment. By means of this procedure one could determine whether the process of rendering judgments can or cannot be conditioned to a subliminal stimulus.

(Manuscript received October 15, 1932)

THE INFLUENCE OF CERTAIN PRIOR CONDITIONS UPON LEARNING *

BY PAUL L. WHITELY AND GERALD BLANKFORT

Franklin and Marshall College

It is a common assumption that the learning process is influenced by such illusive bodily and mental conditions as moods, attitudes, or sets, but the problem of devising a laboratory technique for controlling these conditions has proved particularly difficult. In two previous investigations,[1] one of the writers attempted to exercise some degree of control over the bodily and mental attitudes with which the subjects approached a given task by introducing prior to learning and to recall certain mental and physical warming-up exercises or by attempting to arouse a certain apperceptive system, set, disposition, or trend prior to learning and to recall. The objective data of these experiments seemed to point unequivocally to the fact that the prior conditions mentioned had little or no influence upon learning, but that, under certain conditions, these same factors may influence subsequent recall.

The purpose of the present investigation was to obtain further data upon the relation of conditions prior to learning efficiency, and to employ a variety of conditions antecedent to the learning test. The purpose of the prior conditions was to arouse a certain emotional mood or set, and two different techniques were employed. In the first instance, we attempted to arouse an emotional set of joy or of sadness, by introducing prior to learning two different kinds of stories. The first kind, adapted from All Quiet on the Western Front, by Erich Maria Remarque, and Beyond the

* The writers are indebted to Mr. William T. Roever for assistance in collecting a part of the data.

[1] P. L. Whitely, The dependence of learning and recall upon prior mental and physical conditions, *J. Exper. Psych.*, 1924, 7, 420-428. ————————, The dependence of learning and recall upon prior intellectual activities, *J. Exper. Psych.*, 1927, 10, 489-508.

Wire, by Will R. Bird, in *Colliers* for April 11, 1931, was quite sad and depressing in its nature. The second, adapted from The Social Life of a Newt, by Robert C. Benchley, and A Manual of the New Mentality, by Stephen Leacock, in *Harpers Magazine* for March, 1924, was humorous and sardonic in essence and character. In the second technique, the writers attempted to arouse the moods of joy or of sadness by presenting prior to learning a series of pictures.[2] The humorous pictures were carefully chosen from current magazines of humor, whereas the sad or depressing pictures comprised several of the classic pictures of Jesus, depicting the crucifixion scenes, and other pictures connotative of sorrow or suffering. The pictures, both of sorrow and of humor, used in this experiment were selected from a larger group of pictures by a committee of twelve to fifteen students of psychology in order to assure somewhat universal appeal with respect to the indicated conditions.

Two experiments were conducted, and the details with respect to procedure and technique are presented in connection with the discussion of each experiment separately.

First Experiment

In the first experiment, the stories were used as prior conditions, and three different kinds of learning were employed; namely, letter-digit substitution, the learning of monosyllabic words, and of poetry. In the case of the letter-digit substitution and poetry, the stories were read by the subjects, whereas in the case of the monosyllabic words the stories were read by the experimenter, which occupied about eight or ten minutes of time.. Immediately following the reading of the story, the subjects learned the material presented to them.

1. *Letter-digit Substitution.*—The usual form of letter-digit substitution test was employed in this phase of the experiment. Each of the eighteen subjects, participating in the experiment, performed the substitutions under three different conditions—

[2] The writers are indebted to Professor Knight Dunlap for suggesting this procedure.

with no control over the activity preceding learning (normal), with the humorous story preceding learning (joy), and with the depressing story preceding (sad)—in a counterbalanced practice order. The data for this aspect of the experiment are presented in Table I. The table shows the mean number of correct substitutions, the sigmas of the distribution and the sigmas of the mean, and also the mean number of errors, for the

TABLE I

LETTER-DIGIT SUBSTITUTION

	Normal	Joy	Sad
Mean no. correct	98.60	90.50	90.50
Mean no. errors	.39	.32	.77
σ dis.	28.50	35.50	30.22
σ M.	6.72	8.36	7.12

three conditions of learning. It will be observed that the mean number of substitutions for the normal condition is 98.6, and 90.5 for the joy and sad conditions.

The reliability of the data is shown in Table I*A*. In the five successive columns are shown, respectively, the conditions compared, the differences of the means, the sigmas of the difference, the ratio between the difference of the means and the sigmas of the difference, and the chances in 100 that the

TABLE I *A*

DIFFERENCES OF MEANS AND SIGMAS OF DIFFERENCE[3] FOR THE DATA IN TABLE I

Conditions Compared	Diff. M.	σ Diff.	D.M./σ Diff.	Chances in 100
Normal-joy	8.05	7.29	1.11	86
Normal-sad	8.05	6.64	1.21	88
Joy-sad	0.00			

[3] All sigmas of the difference in this paper were computed by the long formula where the coefficient of correlation between the two series of scores is taken into account. See Garrett: Statistics in Psychology and Education, p. 287, 69b.

true difference is greater than zero. It will readily be observed that the differences do not meet the statistical criterion of reliability ordinarily accepted of three times the sigma of the difference, or complete reliability; however the differences between the normal and joy conditions, and the normal and sad conditions, do show a fairly high degree of reliability.

2. *Poetry.*—The learning material used in this aspect of the experiment was four stanzas of Gilpin's Ride to be found in the Notes for Instructors to accompany Experiments in Psychology by Foster and Tinker. Twenty-four subjects participated in the experiment and learned the poetry in a counterbalanced practice order under the three conditions indicated above. The poetry was scored by determining the average number of lines learned for the various conditions, and account was taken of four different kinds of errors; namely, omissions, misplacements, additions, and substitutions. Three minutes were given to each subject for learning, and sufficient time was given to each subject to write his recall immediately following learning.

Table II, in which are presented the data, shows the

TABLE II

LEARNING OF POETRY

	No. Lines	Errors				
		Omis.	Misp.	Add.	Subst.	Total
	Normal					
Mean	7.79	2.00	1.13	1.58	4.96	9.67
σ dis.	3.09					
σ M.	.63					
	Joy					
Mean	6.79	1.25	.46	.96	4.71	7.38
σ dis.	2.29					
σ M.	.45					
	Sad					
Mean	7.13	2.13	.96	.79	5.92	9.80
σ dis.	3.01					
σ M.	.61					

average number of lines learned, the sigmas of the distribution and the sigmas of the means, and the average number of errors for the three conditions of learning. The average number of lines memorized for the three conditions is as follows: normal 7.79, joy 6.79, and sad 7.13. There are fewer lines learned under the joy condition than either of the two other conditions, and the variability is considerably less. With reference to errors, it will be observed that consistently

fewer errors are made under the joy condition than under the normal and sad conditions.

Table II *A* shows the reliability of the data. The greatest reliability is shown in the difference between the normal and joy conditions, where the ratio between the difference of the means and the sigma of the difference is 1.54, or 93 chances in

TABLE II *A*

DIFFERENCE OF MEANS AND SIGMAS OF THE DIFFERENCE FOR THE DATA IN TABLE II

Conditions Compared	Diff. of M.	σ Diff.	D.M./σ Diff.	Chances in 100
Normal-joy	1.00	.65	1.54	93
Normal-sad	.66	.52	1.27	89
Joy-sad	.34	.69	.49	69

100 that the true difference is greater than zero. The smallest reliability is shown between the joy and sad conditions, the ratio between the difference of the means and the sigma of the difference being only .49. The ratio for the normal and sad conditions is 1.27.

3. *Monosyllabic Words.*—Twenty-four subjects participated in this phase of the experiment, and two minutes were given for learning in a counterbalanced practice order. Each list of words, twenty in number, and consisting of three- and four-letter words, was typed on a sheet of paper and placed face down before the subject. When the story was presented as a prior condition, the subject was instructed to turn the sheet over upon hearing the 'Go' signal, immediately following listening to the story, and to memorize as many of the words as possible in the time allotted. The subject was further instructed that, at the expiration of the two minutes given for learning, he was to turn the sheet over and write on the back of the sheet as many of the words as he could recall. For the normal condition, the procedure was the same, save for the fact that the sad or humorous story was not read.

The mean number of words learned, the sigmas of the distribution and the sigmas of the means, and the average number of errors, are presented in Table III. The mean number of words learned under the normal condition was 11.00, while the means for the joy and sad conditions are 10.62 and 10.08,

TABLE III

LEARNING OF MONOSYLLABIC WORDS

	Normal	Joy	Sad
Mean correct	11.00	10.62	10.08
σ dis	2.69	2.59	2.39
σ M	·55	·53	·49
Ave. no. errors	·54	·33	1.16

respectively. For this type of learning, it is interesting to note that the subjects learn less under the sad condition and make more errors, although there is slightly less variability of their scores. The error scores, incidentally, are consistent with the error scores in Table II, since fewest errors are made under the joy condition and most errors are made under the sad condition. The reliability of the data are to be found in Table III *A*.

TABLE III *A*

DIFFERENCES OF MEANS AND SIGMAS OF THE DIFFERENCE FOR THE DATA IN TABLE III

Conditions Compared	Diff. of M.	σ Diff.	D.M./σ Diff.	Chances in 100
Normal-joy	·38	·44	.86	80
Normal-sad	·92	·51	1.80	96
Joy-sad	·54	·52	1.04	85

The results of these experiments are not wholly equivocal. The differences of the mean scores for the various conditions are small and do not possess a high statistical reliability. However, it will be noted that the normal condition is consistently the best condition in the three phases of the experiment. The data do not seem to justify any definite statements with regard to the relative influence of the joy and sad conditions. In the letter-digit substitution test, these two conditions reveal the same mean scores; in the learning of poetry, the sad condition is slightly better than the joy condition; while the joy condition is slightly better than the sad condition in the learning of monosyllabic words.

SECOND EXPERIMENT

In the second experiment, as has previously been intimated, the presentation to the subject of two kinds of pic-

tures, the one depicting sorrow or suffering, the other humor or gayety, was employed as a technique of arousing a particular kind of mood or set. Five minutes were allotted for the presentation of the pictures, in serial fashion, and the subjects were instructed to endeavor to catch the spirit of the pictures presented. Moreover, the subjects were further instructed that later they would be asked to name the picture which was most connotative of sorrow or of joy or laughter, in order that they would consider the presentation of the pictures an integral part of the experiment.

The learning material consisted of three kinds of words: (1) those connotative of suffering and dejection; (2) those indicating joy or hilarity; and (3) those which we have designated as neutral with reference to affective tone. The words were chosen on this basis by a committee of twelve people. Three lists of each kind were prepared, and fifteen subjects learned the lists under the three conditions of normal, joy, and sad, in a counterbalanced practice order. Three minutes were allowed for the learning of each list. And two minutes were permitted for immediate recall, which was obtained in the manner of written reproduction.

The data for this experiment are to be found in Table IV. In the table are to be observed the means, the sigmas of the distribution and the sigmas of the mean, and the mean number of errors, for the three conditions of learning, and for the three kinds of words and the totals. The reliability measures are to be seen in Table IV *A*.

The results are somewhat ambiguous and difficult of interpretation. Considering the totals first, we find that the differences of the means for the three conditions of learning are, indeed, slight, and have low statistical reliability, the sigmas of the difference being in each case larger than the differences of the means. Likewise, the differences of the mean number of errors do not appear significant.

However, there is a slight indication that the different conditions of learning produce a differential effect upon the kind of words learned. For the neutral words, the normal condition is the best, with the joy and sad conditions holding

TABLE IV

SHOWING RESULTS OF THE SECOND EXPERIMENT

	Prior Conditions		
	Normal	Joy	Sad
	Neutral Words		
Means	12.00	11.33	11.26
σ dis.	2.19	2.31	2.35
σ M.	.56	.59	.59
Ave. no. errors	.46	.60	.66
	Sad Words		
Means	11.53	12.73	12.06
σ dis.	2.55	2.67	1.81
σ M.	.68	.69	.49
Ave. no. errors	.40	.93	1.00
	Joy Words		
Means	9.73	9.66	10.20
σ dis.	1.69	2.72	2.25
σ M.	.38	.70	.57
Ave. no. errors	.62	1.13	.60
	Totals		
Means	11.09	11.24	11.17
σ dis.	2.56	2.51	2.05
σ M.	.38	.37	.35
Ave. no. errors	.62	.88	.75

TABLE IV *A*

DIFFERENCES OF MEANS AND SIGMAS OF DIFFERENCE FOR DATA IN TABLE IV

Conditions Compared	Diff. of M.	σ Diff.	D.M./σ Diff.	Chances in 100
	Neutral Words			
Normal-joy	.67	.62	1.09	86
Normal-sad	.74	.66	1.12	86
Joy-sad	.07	.72	.09	54
	Sad Words			
Normal-joy	1.20	.83	1.44	93
Normal-sad	.53	.62	.85	80
Joy-sad	.67	.72	1.22	88
	Joy Words			
Normal-joy	.07	.62	.11	54
Normal-sad	.47	.50	.81	79
Joy-sad	.54	.81	.68	76
	Totals			
Normal-joy	.15	.39	.38	65
Normal-sad	.08	.33	.24	60
Joy-sad	.08	.34	.23	60

second and third positions, respectively. With the joy words, this relationship is reversed, the greatest amount of learning occurring under the sad condition, the normal and joy conditions occupying second and third positions, respectively. In the case of the sad words, most is learned under the joy condition and least under the normal condition, with the sad condition holding an intermediate position.

The error scores present no very consistent relationship, however it may be concluded that, on the whole, the normal condition is the most favorable one from the standpoint of fewness of errors, the fewest number of errors being made under the normal condition in the case of the neutral words and sad words, and also with the totals.

Discussion of Results

A question may be raised as to the possible effects of the prior conditions utilized in this experiment. Instrumentation was not employed to ascertain the nature of the bodily changes, which took place; however verbal reports were obtained from the subjects regarding the possible influences of the prior conditions upon their mental processes. Here it must be stated that the reports were somewhat discrepant. Some subjects reported that the story, for example, initiated greater mental activity, which made learning easier. Too, certain subjects reported that learning was facilitated by way of associative connection between the story and the learning material, although this connection would not be an obvious one to the experimenters. Some individuals reported no observable effect. However, perhaps a larger number of the subjects reported that, because of the perseverative tendency of the story, concentration upon the learning material was made more difficult. Of course, it is altogether possible that there might be individual differences with regard to the influence of the prior conditions, and the true effects somewhat obscured in the group averages.

From general observation, it was apparent that in part one of the experiment there were different physical reactions in response to the two designated conditions or attitudes.

Clearly could there be distinguished differences in respiration and general facial expressions in the sad and joy attitudes. In the sad condition, for example, one could discern a slow, pronounced, and deep respiration. In some instances, the eyes seemed to be more heavily laden than normal with what appeared to be a thin film. Some subjects exhibited a tendency to run their fingers through their hair as one does in an attitude of pensiveness, or when sad or distressed. On the other hand, there was generally observable in the joy condition a more positive countenance. There was less uneasiness about the person, other than quicker respiration, possibly symbolizing eagerness.

Verbal reports from the subjects with regard to the influence of the prior conditions on learning, revealed such introspections as the following: "sad story hindered learning as it crept in at times"; "words were not clear due to the influence of the sad story"; 'hard to get started'; "mind wandered"; 'comparative ease in learning those words of close relationship to the story'; 'easier to concentrate because of the story,' etc.

So far as objective results are concerned, they indicate, in general, a somewhat inhibitive influence of the various sets upon learning, and under similar conditions of prior and later learning, they suggest what might be called a *proactive inhibition*. However, perhaps the term *proactive inhibition* might better be reserved to designate the detrimental influence of a condition introduced prior to learning upon a subsequent recall, thus differentiating it from a condition which operates inhibitively upon recall when introduced between learning and recall, or retroactive inhibition.

Obviously the need is for further experimental work to determine more precisely the effects of the sets or prior conditions upon learning functions. However, there arises the suggestion, particularly from the results of the first experiment, of the existence of some kind of an inhibitive factor, whether it be affective or purely cognitive in its operational status, or whether it be a function of the difficulty in shifting from one set to another and different kind of set.

Summary and Conclusions

This study was concerned with the problem of the influence of prior conditions upon learning. Two different techniques were employed in an endeavor to arouse two kinds of emotional set or mood; namely, that of sorrow or dejection and of joy or jubilation. In the first experiment, three kinds of learning material were employed—letter-digit substitution, poetry, and monosyllabic words. In the second experiment, three types of words were utilized, which differed in each instance with regard to affective tone.

The results are not wholly unequivocal, but the following tentative conclusions may be deduced from the objective data:

1. When using a story as a means of arousing a set of joy or of sorrow, the best learning is obtained under the normal condition for the three kinds of learning material. No definite statements may be made with respect to the relative influence of the joy and sad conditions, considered alone.

2. With reference to the utilization of pictures as a means of arousing an emotional set, the total scores reveal no significant differences for the three conditions of learning. However, there is a slight indication that the different conditions produce a differential effect upon the kind of words learned.

(Manuscript received October 13, 1932)

QUANTITATIVE AND QUALITATIVE ESSENTIAL ELEMENTS OF TRANSFER

BY FRED McKINNEY

University of Missouri

The many experiments which have been conducted to study transfer fall under such headings as cross education, evidence against formal discipline, interference, generalization or concept formation and equivalent stimuli. Hunter (**3**) has reviewed much of this work and comes to the conclusion that "At present the theory of identical elements seems most adequate, and yet it is all but impossible to bring the theory to a rigorous experimental test." The most recent group of experiments on transfer, the equivalent stimuli experiments, Lashley (**5**) (**6**), Klüver (**4**) and an earlier study by the author (**8**), have thrown some doubt on the atomistic generalization that transfer takes place in terms of identical elements. Orata (**9**) has critically examined the literature and also takes issue with the identical element theory. Carr's (**1**) generalization that "in a very general way it may be said that transfer will occur only between two rather similar situations" seems to be the safest one at the present time. It is, then, the problem of future research to investigate the *nature*, or definition of the limits of this similarity.

Hollingworth (**2**) points out and gives some experimental evidence with the use of advertisements that there are certain prepotent details of a stimulating situation which are more effective than others in evoking a response. These details are probably also more effective when they are the elements in the transfer situation, and transfer is probably not purely a matter of the quantity of identical elements but also depends upon what particular elements are present. Yum (**10**) in a recent experiment, has varied parts of the stimulating situation in an attempt to analyze the efficacy of different parts of stimuli in transfer. Some slight indication that transfer is not only a

matter of quantity of variation but also a matter of the part of the stimulus that is varied can be seen from Yum's raw data.

It is the purpose of this present paper to test systematically the hypothesis that transfer is in terms of the qualitative aspects of the features common to original and transfer stimuli as well as the quantitative aspect, and to ascertain some of these prepotent details of the transfer situation. Phrased in other words, the object is to investigate the quantitative and qualitative nature of an adequate transfer stimulus; to ascertain how much and what details of a given situation are adequate to evoke a given response.

Method

The subject was given four cards measuring six inches by four inches, each card having an accurately drawn geometric figure on it. Under the figure on the first card was the letter M, under the figure on the second card, the letter T; under the third the letter X; and under the fourth, the letter S. The figure M consisted of an eight-centimeter vertical line bisected symetrically by a four-centimeter horizontal line. Thus the figure looked like a cross with one arm missing. The figure X was a two-centimeter vertical line, the figure S a two-centimeter horizontal line, the figure T an eight-centimeter vertical line. All figures were in the center of the cards. The subject was instructed to associate these patterns and letters and was given sixty seconds in which to do this. After fifteen seconds rest the strength of these associations was tested by presenting the subject with a pack of twenty-four shuffled cards. Each card had one of the four figures on it and omitted the letter. Thus, each figure was represented six times. The cards were presented by hand at the rate of five seconds each and the subject responded by recording the letter associated with the figure in the proper space on a sheet of paper given to him by the experimenter. After he had responded to all of the stimuli, he rested for thirty seconds while the experimenter checked his responses. If they were all correct he was allowed to go on with the rest of the experiment; if not, he was again given the four cards with the figures and letters. This time he was allowed to study the cards thirty seconds and after a fifteen-second rest period, the strength of the association was tested. The subject repeated this process of learning and recalling until he could respond perfectly to the pack of twenty-four cards.

When the subject succeeded in associating the letters and figures correctly, he went on with the remainder of the experiment and was given the first altered series. The altered series also consisted of a pack of twenty-four cards, six of each figure, and was presented as the original test series; but it differed in that the figure M was altered either 10, 20, or 30 percent. The alteration consisted of a reduction of: (1) either one of the three arms of the figure; (2) reduction of two arms of the figure simultaneously; or (3) reduction of three arms of the figure simultaneously. For example, one of the altered series consisted of a figure similar to M in all respects except that the lower arm or bottom of the figure was 10 percent shorter. This type of alteration would come under (1). Number (2) can be illustrated by a figure that resembled M in all respects except 5 percent was taken off the top and bottom of the figure (a total reduction of 10 percent in terms of the entire figure). The same figure M with 3⅓ percent taken off the

side arm would be an illustration of (3). There were fifteen of these altered series. The types of alteration are as follows:

A. 10 percent off side arm.
B. 10 percent off lower arm.
C. 3⅓ percent off each arm or 10 percent total reduction of figure.
D. 20 percent off upper arm.
E. 10 percent off upper and lower arm or 20 percent total reduction of figure.
F. 30 percent off side arm.
G. 30 percent off lower arm.
H. 10 percent off each arm or 30 percent total reduction of figure.

This series was given to 40 subjects.

A'. 10 percent off upper arm.
B'. 5 percent off upper and lower arm.
C'. 20 percent off side arm.
D'. 20 percent off lower arm.
E'. 6⅔ percent off each arm or 20 percent total reduction of figure.
F'. 30 percent off upper arm.
G'. 15 percent off upper and lower arms.

This series was given to 35 different subjects.

In regard to the altered series the subject was instructed to respond as he had done in the original series by recording the letter he had learned to associate with the figure. He was further informed that if at any time he was presented with a figure which was different from the one originally learned, he was to record a zero in the proper blank on the paper given to him. This statement was repeated twice to impress firmly the subject with this precept. If the subject asked questions regarding any other parts of the card as slight unavoidable differences in cards that may have resulted from use, he was told to respond only to the figure itself.

One group of subjects was given seven altered series and another eight. They were given the above instructions, a training series and a test on the original series before each altered series was presented. After the third training series, the time of presentation of the four original stimuli was reduced to 15 seconds, as by this time the association was fairly well established. Seventy-five subjects, college students, who were naïve in respect to the experiment, were used. The first results are based on these subjects. We shall call this experiment I. Later the experiment was duplicated with twenty-nine new subjects. The data for this group is recorded separately and serves as a check for the first group. It was collected by another experimenter.[1] We shall call this experiment II.

The different series were presented to the subject in the counter-balanced practice order, and in this way, the effect of practice or previous experience was determined and eliminated.

Results

Table I includes data on seventy-five subjects plus twenty-nine subjects; the latter being used as a check on the former. The table shows the average percentage of transfer under each variation of stimulus. These percentages were arrived at by

[1] Miss Elizabeth Schlecht, an advanced student in the department.

calculating the percentage of times the subject responded to an altered form of M as M and not as a new stimulus in each series. It is clear that in these cases the subject is using a previous mode of response, writing an M, when the figure is no longer M but M altered in some manner. Thus transfer is taking place from M to some altered form of M. The following facts can be seen from this table:

1. The percentage of transfer decreases as the amount of alteration increases providing the locus of alteration remains constant. In other words, as the reduction of any one arm of the figure is increased the percentage of transfer decreased. This can be seen by comparing the averages of the percentages of transfer to the stimuli with 10 per cent, 20 per cent and 30 percent reduction with each other. However, this decrease in transfer is not proportional to the increase of alteration. The decrease in transfer appearing when the percentage of transfer to a stimulus with a 20 percent reduction is compared with that to a stimulus with a 30 percent reduction is greater than the decrease appearing when the transfer to a stimulus with a 10 per cent reduction is compared with one with a 20 percent reduction. This decrease is likewise greater than that which appears when the percentage of transfer to a stimulus with a 10 per cent reduction is compared with the transfer to the original stimulus which is not shown in the table but which is slightly below the 100 percent mark. These results substantiate the generalization that transfer is not strictly proportional to the amount of identical material present.

2. The amount of transfer present in a given situation is a function of the locus of the alteration, a 10 percent reduction of the stimulus, for example, yielding different results depending upon the part of the stimulus in which the alteration is made. The fact should be noted that in the case of the 30 percent reduction in the first set of data the transfer was as low as 12.30 percent and as high as 81.60 percent, showing that the degree of transfer in the case of asymmetrical patterns approaches zero while the symmetrical patterns with the same amount of reduction still yield a higher degree of transfer. This suggests that certain quantities of identical stimulation

may yield no transfer whereas the same quantities arranged in a different qualitative relationship will result in transfer.

3. This factor of locus of alteration is more effective as the quantity of the reduction is increased—but not proportionately so, the 30 percent reduction showing not much more advantage for this factor than the 20 percent.

4. Symmetrical stimuli as represented by the 30 percent reduction, 10 percent taken off each arm, yield a greater amount of transfer than asymmetrical stimuli as those represented by 30 percent off one arm.

5. Symmetrical stimuli are much less affected by quantity reduction or reduction in size in their efficacy in evoking responses in transfer situations. Stimuli E and H, for example, do not show as great a loss of percentage of transfer as do any of the asymmetrical stimuli as F′ and D′.

6. As the symmetry of the stimuli increases, the percentage of transfer tends to increase. Those stimuli on which only the upper and lower arms are reduced show more transfer than those on which only one arm is reduced. Those having all arms reduced show more transfer also than those with just one arm reduced and in some cases more than those with just upper and lower arm reductions.

7. Symmetry is three times as effective as asymmetry as measured by this experimental set-up. A 30 percent symmetrical reduction is as effective as a transfer stimulus as a 10 percent asymmetrical reduction.

8. Symmetry is more effective than size as a factor in transfer, some 20 percent and 30 percent reductions yielding more transfer by this measure than some 10 percent reductions.

Table I also shows the sigmas of the above measures or, in other words, the variability of transfer scores of the group of subjects to the different stimuli. Variability may be used as another method of measuring transfer, in that, it shows the consistency or lack of consistency of response by the subjects to various stimuli. Lack of consistency can be interpreted to mean absence of a tendency to transfer to the stimulus. Table I shows the following facts concerning the variation in response to the different transfer stimuli:

TABLE I

PERCENTAGE OF TRANSFER AND VARIABILITY OF TRANSFER RESPONSES

Series of Stimuli	Locus of Reduction	Size of Reduction of Figure in Percent	Average Percentage of Transfer		Variability of Transfer Responses in Terms of dis.	
			Exper. I	Exper. II	Exper. I	Exper. II
B	Lower arm	10	91.60	89.57	7.45	16.99
A′	Upper arm	10	69.90	94.04	22.20	8.87
A	Side arm	10	89.90	74.99	6.95	22.52
Average	Single arm	10	83.80	86.20	12.30	16.12
B′	Upper and lower arm	10	95.20	98.80	5.60	2.87
C	All arms	10	95.80	95.75	4.95	11.42
Average	All	10	88.48	90.63	9.42	12.51
D	Upper arm	20	66.60	66.66	23.70	34.92
D′	Lower arm	20	26.30	63.03	24.00	38.75
C′	Side arm	20	50.90	73.80	29.50	40.36
Average	Single arm	20	47.90	67.83	25.70	38.01
E	Upper and lower arm	20	98.30	93.75	3.48	16.52
E′	All arms	20	93.30	85.71	7.77	19.10
Average	All	20	67.08	76.59	17.69	29.93
G	Lower arm	30	20.80	39.58	21.10	30.26
F′	Upper arm	30	12.30	24.98	16.00	22.28
F	Side arm	30	41.20	43.75	18.00	39.03
Average	Single arm	30	24.70	36.10	18.36	30.52
G′	Upper and lower arm	30	53.30	79.74	22.50	19.85
H	All arms	30	81.60	82.28	15.30	24.28
Average	All	30	41.84	54.06	18.58	27.14

1. Variability of response increases as the amount of alteration increases providing the locus of alteration remains constant. This can be seen by comparing the averages of the transfer score sigmas of the stimuli with 10 percent, 20 percent and 30 percent reduction with each other.

2. Variability of response to a given situation is a function of the locus of the alteration. The variability for stimuli with alterations on only one arm is higher than those with alterations on two arms and three arms. This corroborates the results shown by the table on the percentage of transfer.

3. The factor of locus of alteration becomes more effective as the reduction is increased from 10 to 20 percent, as for example those stimuli with alterations on only one arm show greater differences in variability comparing the 10 percent and 20 percent alteration than those with alterations on two and three arms. No advantage is apparent for this factor when

the figure is reduced 30 percent as measured in terms of the variability of response of the subjects. This partially agrees with the data on the percentages of transfer. The reason that this does not agree completely with the other data is evident when one considers that when the figure becomes so asymmetrical in the case of some of the 30 percent reductions that there is little doubt that the figure is different from the original and thus the degree of agreement between subjects increases. Most of the subjects under these circumstances are just as sure that the stimulus should not elicit the same response as the original did as they are that it should when the figure is symmetrical.

4. Symmetrical stimuli yield a greater amount of transfer than asymmetrical stimuli except in terms of the 30 percent reduction as measured by the variability of response. The failure of the 30 percent reduction to agree is explained above and is obvious when one consults the figures themselves.

5. The finding mentioned above that symmetrical stimuli are less affected than asymmetrical stimuli in their potency in evoking transfer by a reduction in size is substantiated by the data on the variability of response except for the 30 percent reduction. This can be seen by comparing figures B′ and C with figures B, A′ and A and figures E and E′ with figures D, D′ and C′.

6. As the symmetry of the stimuli increases the percentage of transfer increases. For example the figures with alterations on two sides and three sides show less variability of response than those on only one arm.

7. Symmetry is more effective than size as a factor in transfer as measured by the variability of response. This can be seen by comparing any of the stimuli with a reduction of 10 percent in a single arm with any of the stimuli with a 20 percent equal reduction on two or three arms. The case of 30 percent reduction is the one exception and the reason for this is explained above.

It can be seen that in using variability of response as a measure of transfer we arrive at the same generalizations as in using percentage of transfer as the measure. The agreement strengthens the validity of the generalizations.

Discussion

What do these facts mean in terms of the current theories of transfer? First, they clearly demonstrate the fact that the theory of identical elements is limited in its possibilities of explaining the phenomena of transfer. It shows that quantity of identity is conditioned by a more important factor quality of identity, or to put the matter in terms of the Gestalt psychologist, the identity of pattern is more effective than the identity of elements as a transfer stimulus. Data show that this qualitative factor [2] becomes more important as the quantity of the identical stimulus decreases. Further, identical elements may be omitted in greater quantities when the stimulus is symmetrical than when it is asymmetrical. Also, certain quantities of material which produce practically no transfer can be made to produce a large amount of transfer by arranging these quantities in certain qualitative relationships. This all seems to point to the importance of the qualitative restrictions which must be placed on any quantitative identical element theory of transfer if such a theory is at all feasible.

The other less mechanistic theory of transfer has been placed in terms of generalization by some and in terms of configuration by others. The identity here is in terms of meaning. Two stimuli may be different physically but may arouse the same meaning and therefore evoke the same response. The findings presented here seem to substantiate more this type of theory than they do the atomistic identical element theory. As long as the stimulus maintained enough of its original proportions, it was judged by a large percentage of the subjects to be the same stimulus. However, as soon as the pattern of the stimulus is changed in the process of alteration, even though there were more identical elements than there are in a stimulus in which the pattern had not undergone a change, transfer is less. The change in pattern results in a different meaning and therefore the similarity of meaning is absent and the particular response is not elicited.

[2] By qualitative factor is meant the fact that a stimulus is not only a function of its mass or quantity but also the manner in which this quantity is distributed. Quality does not mean something utterly disparate from quantity but rather quantity plus another factor.

The results seem also to establish the fact that symmetry or form is a prepotent element in transfer and that it is more effective as a transfer stimulus than size or quantity of physical identity of stimulus. Its potency increases as the reduction of the identical elements increases.

It is therefore clear that transfer is in terms of the qualitative aspects of the stimulus as well as, and in some cases, instead of, the quantitative aspect. The data show the importance of the qualitative aspect and suggest the possibility of other prepotent aspects of stimuli which may be more capable of instigating responses than some other parts of stimuli. It is only through the isolation of such factors that we will arrive at a definition of the limits of the similarity factor which is responsible for transfer, and such an analysis is important for an understanding of the nature of transfer. The gross studies supply us with interesting data and data which have practical implications but most of them tell us little concerning the ultimate and internal nature of transfer. They do nothing in the way of defining specifically the conditions or laws of transfer.

These data also bear on the problem of the effect of the change of sensory situation on recall. The original series may be regarded as the learning series and that which is called the transfer series may be regarded as the recall series. The generalizations arrived at above in regard to the results are equally true for the effect of change in sensory situation in recall, sensory situation being defined as the entire sensory antecedent of a response. We see the recall decreasing as the change in sensory situation becomes greater. We likewise see that the change in the sensory situation is also conditioned by qualitative relationships. Some components of the situation can be omitted without greatly altering the response, others when omitted alter reactions greatly. We see, for example, from the averages in Table I, that with a 20 percent change in stimulation the reinstatement of the previously learned response will sometimes be decreased as much as 73.70 percent and with a 30 percent change there is a reduction of response as much as 87.70 percent. This is a relatively small

reduction of stimulation considering how indiscriminately we use the concept of identity in our every day life. It is quite conceivable that often in daily instances of forgetting there is a 20 or 30 percent reduction or change of the central stimulus. We perceive the stimulus as being identical with the stimulus originally eliciting the response but it does not have the essential components and therefore does not call out the response. Such a percentage reduction in certain elements, as is seen above, is enough to alter or fail to elicit the original response. There are several studies on the effect of change of sensory situation on response but most of these are concerned with a change in the fringe or context of sensory stimuli rather than the central stimulating condition, which certainly changes greatly under non-laboratory conditions and which might conceivably account for much of forgetting as has been pointed out recently by McGeoch (**7**). It is only in the laboratory (if always then) that a stimulus is the same quantitatively and qualitatively at different times.

Finally, as alluded to above, these data relate to the problem of the amount of cue necessary for redintegration as Hollingworth (**2**) would probably word it or the extent of the operation of the law of assimilation as Carr (**1**) most probably would talk of it. This experiment is very similar to Yum's (**10**) problem of finding the extent to which a motor response which has been contiguously associated with a given stimulus will be aroused by a similar one. It seems to give evidence that the similarity of meaning evoked is the important kind of similarity involved in the operation of the law of assimilation or in the process of redintegration. It substantiates Yum's findings that similarity of two stimulating situations are important conditions of transfer and augments the phrase especially if this similarity is of certain qualitative kinds.

Transfer, assimilation, redintegration and recall, of course, involve the same fundamental process but each is discussed separately here because often they are separated for experimental purposes.

Summary

After learning to respond perfectly to four very simple geometric figures, 104 subjects were presented with altered

forms of one of these figures. The alterations varied from 10 to 30 percent in precise reductions and from perfect symmetry to apparent asymmetry. Transfer to these forms was measured in terms of percentage of response to them as the original stimulus. The results show the percentage of transfer to increase with the amount of alteration but not proportionally with it. Also transfer is shown to be a function of the part of the stimulus altered or the locus of alteration. Symmetrical stimuli yield much more transfer than asymmetrical stimuli of the same percentage of reduction and are less affected by quantity of reduction. These results are interpreted as contradictory to a strictly identical element theory of transfer and in agreement with a theory advocating meaning as the condition of similarity between an original and transfer situation. The data show further the pronounced effect of a relatively small change in stimulating situation on retention, and they substantiate qualitative factors as important in the operation of cues in redintegration.

(Manuscript received November 14, 1932)

References

1. Carr, H. A., Psychology, New York: Longmans, Green, 1926.
2. Hollingworth, H. L., Psychology: Its Facts and Principles, New York: D. Appleton and Co., 1928.
3. Hunter, W. S., Learning: II. Experimental studies of learning, Chapter 15, The Foundations of Experiment Psychology, edited by Carl Murchison, Worcester, Mass., Clark University Press, 1929.
4. Klüver, Heinrich, The equivalence of stimuli in the behavior of monkeys, *J. Gen. Psychol.*, 1931, **39**, 3–27.
5. Lashley, K. S., Learning: I. Nervous mechanisms in learning, Chapter 14, The Foundations of Experimental Psychology, edited by Carl Murchison, Worcester, Mass., Clark University Press, 1929.
6. Lashley, K. S., The mechanisms of visiôn: I. A method for rapid analysis of pattern vision in the rat, *J. Gen. Psychol.*, 1930, **37**, 453–460.
7. McGeoch, J. A., Forgetting and the law of disuse, *Psychol. Rev.*, 1932, **39**, 352–370.
8. McKinney, F., Identical sensory elements versus functional equivalents in visual transfer reactions, *J. Gen. Psychol.* In Press.
9. Orata, P. T., The Theory of Identical Elements, being a critique of Thorndike's theory of identical elements and a re-intepretation of the problem of transfer of training, Columbus, Ohio, The Ohio State University Press, 1928.
10. Yum, K. S., An experimental test of the law of assimilation, *J. of Exper. Psychol.*, 1931, **14**, 68–82.

THE EXPERIMENTAL CONTROL OF VISUAL FACTORS [1]

BY T. H. HOWELLS AND T. H. CUTLER

University of Colorado

In many studies of visual perception it is necessary, or at least desirable, that the various physical factors in vision be under control in such a way as to permit variation of each factor independently of every other factor. In monocular vision, for instance, it is evident that three very important physical factors are (1) size of retinal image, (2) distinctness of the image, (3) adjustment of the focus of the crystalline lens. Independent variation of these factors is especially difficult for the reason that variation of one of the factors usually affects the other two. It is possible to vary the size of the retinal image by moving the object to or from the eye but there has been no technique available in the past which has made it possible to vary this size factor without affecting either the distinctness factor on the one hand, or else the lens-adjustment factor on the other. Ordinarily when an object is brought closer to the eye maximum clearness is maintained through a corresponding variation in the focal adjustment of the lens through the operation of the ciliary muscles. The usual method of avoiding this variation in lens adjustment is by resort to what is called the fixation procedure.[2] The o is asked to maintain fixation on some stationary object in the visual field, while the stimulus-object is moved either closer or farther from the eye. In using this method however, it is inevitable if the lens-adjustment remains constant that the distinctness factor must vary in inverse ratio as the variation in distance of the stimulus-object from the point of fixation.

It seems manifest that up to the present time there has

[1] The writers are indebted to Dr. F. R. Spencer and Dr. V. P. Lubovich of the University of Colorado for their advice and assistance.

[2] W. Blummfeld, *Zsch. f. Psychol.*, 1913, **65**, 241–404.

been no actual separation of these three visual factors. This seems to be a matter of considerable significance for experiments in visual perception. It is possible that failure to establish such control may throw some question on the validity of certain experiments which attempt to evaluate the various visual factors. In instances in which positive relationships have been observed between visual-psychological phenomena and any one of these factors it cannot be known which of the factors is the causal or concomitant factor, or whether the pattern of association between the factors is the important element. On the other hand in instances in which negative relationships have been reported between various phenomenal variables and changes in values of these physical factors the proof has frequently depended upon the fact that the phenomenal change was observed while the physical factor in question was held constant. With the technique employed, however, it is manifest that holding one of these physical factors constant enforces a changed variability in one or more of the other factors which may account indirectly for the perceptual phenomena. In other words as long as a differential relationship exists between these physical factors, if one of them is restrained its effect is likely to be realized indirectly through its influence on one of the others and the conclusion of negative relationship therefore is not valid.

The general principle involved in the proposed technique is that lens-adjustment shall be independently controlled through auxiliary lenses which are placed in front of the eye.[3] If it is desired, for instance, to vary the size of the retinal image while holding lens-adjustment and distinctness constant, the preliminary procedure would be to move the stimulus-object nearer or farther away from the eye, which would make the retinal image correspondingly larger or smaller. In an uncontrolled situation these changes would necessarily entail corresponding changes in the lens-adjustment if the eye is focused on the object so that maximum distinctness is maintained. The proposed technique would

[3] E. R. Jaensch presents a similar method for equalizing conditions within the eyes. See *Zsch. f. Psychol.*, 1909, **4**, 19–26.

avoid this change in lens-adjustment by introducing in front of the eye an auxiliary lens of such value as to produce, with the same adjustment, maximum distinctness of the image, although with the object at the changed distance from the eye.[4]

As a second option the lens-adjustment can be varied while maintaining a fixed or constant size of image of constant distinctness. In this case the object is kept at a fixed distance while various auxiliary lenses are introduced in front of the eye, each of which forces the eye to make a different focal adjustment in order to secure the best vision. A third possibility would be to hold the lens-adjustment and size of image constant while varying distinctness. This could be done by asking the O to train his eye (without an auxiliary lens) on a fixation-object. The stimulus-object could be fixed any where else in the field of vision and the distinctness of the image could be varied by introducing various lenses between it and the eye (although not so as to obscure the fixation-object).

In order to make such a technique practical and usable it is necessary to know just what effect lenses of different focal-values will have upon the lens-adjustment of the eye and on the distinctness and size of the image. There are two possible methods of determination of these lens values. The first, or the experimental method, is actual trial of the effects of various lenses on the images in a model eye. The second, or mathematical method, would aim to compute the refractive values which must obtain in the auxiliary lens in order to produce a specified effect on the image. The writers considered it desirable to use both methods in order that one might serve as a check on the accuracy and dependability of the other.

The immediate objective of the writers was to establish a series of stimulus situations, each of which could be compared with another or with a constant or standard situation. The purpose was to discover the effect on judgments of the various factors involved (comparative distance, size, etc.) in accord-

[4] This involves a slight change also in the size of the image, which is taken account in the way hereafter described. Size of retinal image is still approximately proportional to distance of the object.

ance with the constant-stimulus method of psycho-physics. The aim was to establish a series of situations in which the factors of lens-adjustment and distinctness would remain constant while step-variations were made in the size of the retinal image.

As a criterion of effects within the actual eye a model-eye was constructed.[5] A glass lens was used with air as the other refractive medium within the model. The value of this system was such as to bring far objects to sharp focus on a translucent screen at a distance of 22 mm. back of the optical center of the lens. This is approximately the distance from the retina to the optical center of the lens system of the eye.[6] It is not important that this value be exactly the same as the average human eye provided the screen is placed at the principal focus of the lens. The reason for this will appear in the later discussion. This model-eye was mounted in the wall of a light-tight box with an illuminated object (11.5 cm in diameter) in front of it, at or near the various distances indicated in Table I. The translucent screen was a piece of kodak

TABLE I

Situation Number	Diameter of Object in cm	Distance of Object in cm	Diameter of Retinal Image in cm	Focal Value of Auxiliary Lens in cm	
				Exp.	Calc.
1.	11.5	52	0.47	0.50	0.52
2.	11.5	77	0.37	0.75	0.77
3.	11.5	101	0.28	1.00	1.01

film with a linear scale photographed upon it. A magnifying glass was mounted back of the screen as an aid in observation of the image and scale. A support was provided for the auxiliary lens at a distance of 15 mm in front of the lens of the eye-model.[7]

With the object at or near each of the distances shown in

[5] A similar model, but for a different purpose, was constructed by G. H. Gliddon. See *Arch. of Opthalmology*, 1929, 2, 138–163.

[6] M'Kendrick and Snodgrass, The Physiology of the Senses, 1893, pages 128–9.

[7] This distance is given by opticians as the optimum distance for distinct vision with the minimum of consciousness of the auxiliary lens.

Table I, various lenses from an opticians test-set were tried until one was found which gave the greatest distinctness of the image. The dioptric strength of this lens for each situation is given in the table under *Focal Value of Auxiliary Lens—Exp.* Since the choice of different powers of lenses was limited it was necessary to move the object a very slight distance from a fifty, seventy-five, one hundred centimeter series to the distances given in Table I, in order to obtain maximum distinctness. The diameter of the image produced on the screen was observed as recorded in Table I. This procedure was repeated for the three test-situations shown in the table.

As a means of checking these results and also of theoretically determining the focal-values of the auxiliary lenses which are necessary to produce distinct images of the sizes given, and with the object at the given distances in front of the eye-

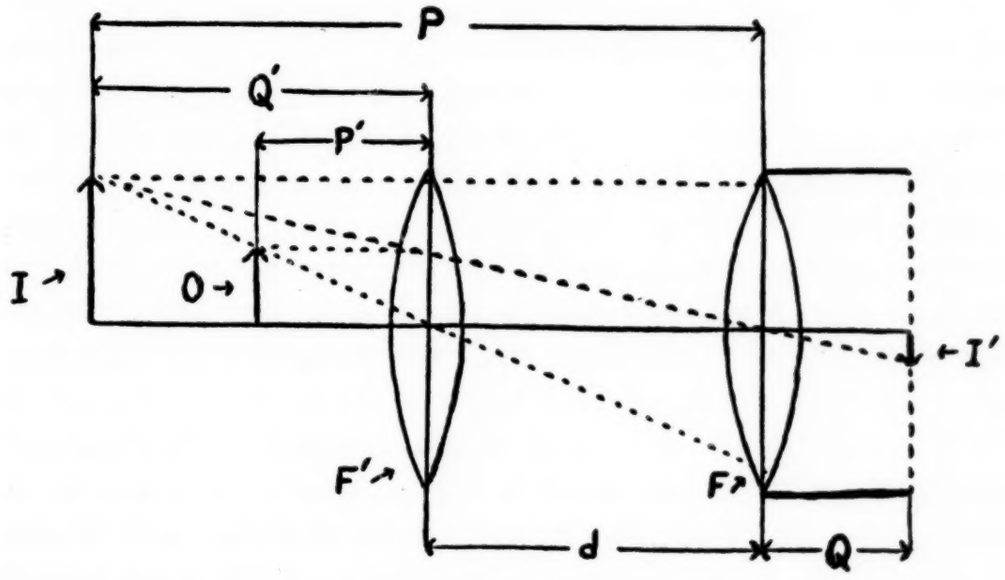

FIG. 1

model, the following formula was developed as applied to Fig. 1.

In Fig. 1, F represents a lens with a focal length equal to that of the eye, while F' represents the focal value of the auxiliary lens; O is the object; I is the virtual image and I' the real image on the retina.

Starting with the standard lens formula, where P is the distance from the lens to the image, where Q is the distance to the object and F is the focal length of the lens,

$$\frac{1}{P} + \frac{1}{Q} = \frac{1}{F},$$

it develops that

$$P = \frac{QF}{Q - F}.$$

But $Q' = P - d$. Substituting for P we have

$$Q' = \frac{QF}{Q - F} - d = \frac{QF - Qd + dF}{Q - F}.$$

Substituting the reciprocal in the formula for F' we have

$$\frac{1}{P'} - \frac{Q - F}{QF - Qd + dF} = \frac{1}{F'},$$

$$F' = \frac{1}{\frac{1}{P'} - \frac{Q - F}{QF - Qd + dF}}.$$

This formula was used in calculating the values of F' when the other values are given. The calculated values are recorded in column *Focal Value of Auxiliary Lens—Calc.* of Table I, while the corresponding experimental values are given in the column *Focal Value of Auxiliary Lens—Exp.* In the special instance of the eye, in which the distance between the retina and lens remains constant, it is apparent the F is always equal to Q. Therefore the second item of the equation $(Q - F/QF - Qd + dF)$ is always equal to zero. Therefore $1/P' = 1/F'$ and $P' = F'$, and the focal length of the auxiliary lens is therefore always equal to the distance of this lens from the object. It follows in practice simply that the focal length of the necessary auxiliary lens must always be equal to the distance at which the object is to be placed from the auxiliary lens.

If it is desired to calculate the size of the retinal image which is produced by an object of a given size, at a given distance, and with a given auxiliary lens, it is possible to use the formula which is developed below.

Let M^1 be the magnification of the auxiliary lens, which is I/O, and M^2 be the magnification of the eye (F), which is I'/I, and M^3 be the magnification of both combined, which is

$$\frac{I}{O} \times \frac{I'}{I} = \frac{I'}{O}.$$

It is evident from the diagram, however, that

$$\frac{I}{O} = \frac{Q'}{P'}, \quad \text{and} \quad \frac{I'}{I} = \frac{Q}{P}.$$

Therefore the combined (multiplied) magnification,

$$M^3 = \frac{QQ'}{PP'}.$$

Substituting for these values the equivalent values previously obtained, and combining,

$$M^3 = QF + \frac{(Q - F)(F' - d)}{FF'}.$$

It is evident that the changes in conditions within the model-eye, which are produced by changing the value of the auxiliary lens used in association with it, are necessarily effected without any change in the lens of the model, and therefore would be similarly produced in the actual eye without any change occurring in the adjustment of the crystalline lens.

While the system as outlined is probably best adapted for use in connection with a series of graded situations, in the way suggested, it should be recognized that it can be used also to produce continually varying situations. In this case it is necessary to use a double auxiliary lens, or rather two auxiliary lenses so arranged that the distance between them can be varied in a certain relationship to changes in the distance of the object. These relative distances can be determined empirically for a series of situations. The distances can be maintained automatically by mounting one of the pair of lenses so that it would be moved by a cam which should be connected by a rod to the object. The curvature of the cam could also be determined empirically. With this arrangement the object could be moved continuously to or from the observer with a corresponding change in size of the retinal image, but without changing either clearness or the adjustment of the crystalline lens.

The distances between the pair of auxiliary lenses and to the object can also be determined mathematically. Where F_1 is the focal length of the outer lens, and F_2 is the focal length of the inner lens (next the eye), and P is the distance from the outer lens to the object, and D the distance between the lenses, it can be shown that

$$P = \frac{F_1(F_2 - D)}{F_1 + F_2 - D}.$$

This formula applies to conditions under which parallel rays are introduced into the eye, and where D is not greater that the focal length of either F_1 or F_2. Where F_1 and F_2 are equal and are referred to as F, the formula becomes

$$P = \frac{F^2 - FD}{2F - D}.$$

It is obvious, of course, that all this technique does is to control the circumstances or mode of introducing rays of light into the visual equipment of the subject. If he has defective eyes and uses glasses these circumstances do not greatly affect the position of the ocular center of the eye. It is obvious also that these complications exist in all experiments in vision since they play a part in the normal visual pattern.

The value of being able to produce these independent variations in visual factors, and to determine their magnitude through both experimental and mathematical means is perhaps apparent. The application of this technique to various experimental situations is a problem for the experimenter in each case.

DISCUSSION

THE INADEQUACY OF THE CONCEPT OF UNILATERAL CEREBRAL DOMINANCE IN LEARNING

BY SHEPHERD IVORY FRANZ

University of California at Los Angeles

Much has been written within recent years regarding unilateral cerebral dominance. The concept has been invoked as an explanation of a variety of phenomena. As far as I have been able to determine the hypothesis is based mainly on two series of facts: the preferential use of the right or the left hand for many activities, and the neuropathological correlates of the aphasias. Of these, the second is by far the more potent argument for a unilateral use, because a supposed definite handedness is not complete except for one or two operations, and for others it may vary from fifty-one to a higher percentage.

Recent experiments, originally planned to determine some of the cerebral conditions in sensory learning, provided data on the relative use of the two cerebral hemispheres in normal humans. The results point to the inadequacy of the hypothesis of a general unilateral cerebral dominance for visual processes, although they may not be adequate to answer questions regarding motor functions. Full accounts of the experimental processes and results, with a discussion of their bearing upon the general problem of the neurology of learning, will be found in four articles.[1] Here I purpose to discuss briefly the relation of some of the results to the widely accepted belief in the domination of one hemisphere in persons who exhibit a motor handedness greater than fifty percent.

The experimental set-up was such that when the eye was fixated

[1] S. I. Franz and J. D. Layman: Studies of Cerebral Function: (1) Peripheral Retinal Learning and Practice Transfer. *U. C. L. A. Publ. Educ. Philos. Psychol.*, 1933, Vol. 1. S. I. Franz and S. Kilduff: Studies of Cerebral Function: (2) Cerebral Dominance as Shown by Segmental Visual Learning. *Ibid.* S. I. Franz and R. C. Morgan: Studies of Cerebral Function: (3) Transfer of Effects of Learning to Other Retinal Areas. *Ibid.* S. I. Franz and E. F. Davis: Studies of Cerebral Function: (4) Simultaneous Reading with Both Cerebral Hemispheres. *Ibid.*

on a dot, or on a small surrounding area, a one-tenth second exposure was made of a much larger area in which there were peripheral visual stimuli, five or more degrees away from the fixation point. These extra-foveal stimuli were arranged in such a fashion that simultaneously one would stimulate the left half of the retina (and cerebrum), another the right half. The stimuli were changed from exposure to exposure, and they were sufficiently numerous and complex so that they were not readily apprehended except after considerable experience. Learning curves were obtained for both halves of one retina (and corresponding cerebral hemispheres).

The learning curves showed that there was usually a difference for the two halves of the retina-cerebrum. Both right- and left-handed subjects showed this result. Of ten 'right-handed' subjects (in two series of experiments) three showed an apparently greater learning ability with the right side of the retina, whereas the other seven followed the supposed rule of left dominance. In another series ten 'right-handed' showed learning ability greater for the left side of the retina, but so did four other subjects in the same series who were 'left-handed.' The foregoing results were obtained when the extra-foveal stimuli were geometric nonsense figures. In a third series in which the extra-foveal stimuli were four letters, which together made a common word but two of which were on either side of the fixation point, six 'right-handed' subjects learned to 'read' more readily the letters which affected the right side of the retina than those which affected the left side.

These results show, therefore, that in visual apprehension and learning there is not that hemispherical dominance that has been supposed. They show, furthermore, that this result is obtained whether one uses meaningless or meaningful material. Although other experimental data may be lacking, common experience seems to bear out this general conclusion. We appear to be able to see an automobile approaching from the left side (and, therefore, with the right halves of the retinae and the corresponding hemisphere) as well as one approaching from the right. We also appear to be able to take in parts of other scenes equally well with both halves of the retinae. Casual observations of this kind may need to be put to an experimental test before they are accepted as scientific facts, but the results of the experiments which are cited above certainly indicate that those who invoke the concept of a unilateral cerebral dominance should not extend its application to all cerebral functioning without further experimentation. Moreover, the exhibition of a definite

handedness, amounting let us say to sixty percent, does not warrant the broad generalizations which have become current.

(Manuscript received July 21, 1933)

A NOTE ON HARTMANN'S EXPERIMENTS SHOWING THE EFFECT ON VISUAL ACUITY OF SIMULTANEOUS STIMULATION OF OTHER SENSE ORGANS

BY CHARLES H. HONZIK

Psychological Laboratory, University of California

In two articles (this JOURNAL, 1933, **16**, pp. 383–407) Hartmann has presented experimental evidence which shows an increase in visual acuity of the right eye with simultaneous auxiliary illumination of the left eye, and also with auditory, olfactory, tactual and pain stimulations. Dr. Hartmann's tentative explanation of these phenomena assumes some sort of cortical diffusion, 'probably through the medium of the so-called association fibers,' so that, for example, if the temporal region is thrown into action by auditory stimulation there is a flow of energy toward the occipital lobe, and visual acuity is thereby increased.

Because of a present tendency to explain many psychological phenomena by reference to supposed cerebral processes, greater care may need to be taken to prevent a return to the earlier unwarrented cerebral hypothesis of Flourens. Dr. Hartmann's suggestiva explanation of his data should be considered in the light of other facts, and also in relation to the inconstancy of the phenomena. Well known facts in physiological optics point to a simpler explanation than that offered by Dr. Hartmann.

We were concerned first to discover whether or not the differences between the means of the normal readings and the experimental readings for each subject were statistically significant. After converting the A.D.'s in the tables to S.D.'s [1] it was possible to apply the formula for the critical ratio $\left(\frac{D}{\sigma \text{ diff.}}\right)$, and thus determine the probability of the observed differences between means occurring by chance. The critical ratios computed from Table I (first article) range in magnitude from .43 to 14.24.

[1] The ratio of A.D. to S.D. is .81 in symmetrical distributions. Holzinger, K. J., *Statistical Methods for Students in Education*, p. 113.

Most of the ratios (13 in a total of 19) are of such magnitude that chance must be ruled out, and we may say that with light in the left eye the visual acuity of the right eye was truly increased. For the other tables (in both articles) these ratios were not computed, but inspection of the means and A.D.'s indicates that these would probably be of the same magnitude. It should be mentioned that the extreme variability in magnitude of the critical ratios computed from Table I, and the rather large differences between the four means of the same subjects as found in the tables, point to considerable variability not only as between subjects but within the performance of individual subjects. This variability may be natural; it may, on the other hand, be due to defects in the instrument used to determine visual acuity. (Beside the two reversals in the magnitude of the normal and experimental means mentioned by the author, one other reversal was found, *i.e.*, for Ma, Black on White series.)

It occurred to the writer that possibly some of the auxiliary stimuli increase visual acuity more than do others. If the 'average differences between means' in the four tables of the first article are taken and the average 'average difference between means' is computed, it is found to be .44. This is with light (auxiliary stimulus) in the left eye. With auditory stimuli (Table I, second article) it is .197; olfactory stimuli (Table II), .35; contact stimuli (Table III), .085; and pain (Table IV), .105. Thus, light in the left eye appears to increase visual acuity more than do the other stimuli; next in order of facilitation is odor, then sound, pain, and finally contact. If one accepts Dr. Hartmann's concept of diffusion of nervous energy from one cortical region to another, this greater facilitation by light in the left eye might be explained by the junction of fibers from each eye beyond the optic chiasma. Due to this junction of fibers each cortical visual center is connected with both eyes. Light in the left eye, therefore, stimulates not only the left visual center but also the right; the addition of nervous energy, if such be the case, may have an effect then directly in the visual centers. With other than visual stimulation, on the other hand, since the corresponding sensory areas are removed some distance from the visual center, diffusion may take place, but less effectively.

However, consideration of the neuro-anatomy of vision and of certain well established facts in physiological optics leads to the conclusion that the increase in visual acuity by auxiliary stimuli may be due to simple reflex action and not to cortical diffusion. Dr.

Hartmann states that "an artificial pupil with a diameter of 1.5 mm. was placed before the subject's right eye to maintain pupillary influences constant" (p. 384). It is doubtful that an artificial pupil holds pupillary influences constant, and certainly it does not if the other eye is stimulated by light. In fact a simple laboratory experiment with an artificial pupil is used to show the change in size of pupil of one eye when the other eye is exposed to, or covered from, the light (consensual pupil reflex) (4, p. 98). It is known also that various other stimuli are able to bring about pupillary changes. Stroking or pinching the skin of the neck calls forth a dilation of the pupil (cilio-spinal reflex). Sudden loud noises and painful stimuli are known to produce pupillary changes.[2] It is therefore not only possible but quite probable that the auxiliary stimuli used in the experiment brought about some changes in the size of the pupil of the right eye.

Among the nine or ten factors usually listed as affecting visual acuity, size of pupil is one. Quoting from Zoethout (5, p. 339), "It will be recalled that the function of the pupil is two-fold: to regulate the amount of light entering the eye, and to limit the amount of spherical aberration always present. The larger the pupil, all other factors remaining constant, the greater the blur circles on the retina and the less distinct the images. A large pupil will, therefore, tend to diminish the visual acuity. But, again, if we largely constrict the pupil, the amount of light entering the eye is decreased, and this may reduce visual acuity." Since pupillary changes, reflexly produced, were very probably present in Dr. Hartmann's experiments, it seems likely that at least some of the changes in visual acuity were due to these pupillary changes.

Although on other experimental grounds (1) we may believe that there is a general diffusion of effects in the cerebral cortex, the present series of experiments seems to be mildly confirmatory rather than demonstrative. A similar but reverse experiment, which is not frequently mentioned, is that of Johnson (2) in which he found that in a minority of his subjects light stimuli increased tactile discrimination. In addition, the recently reported results of Mukherjee (3), showing an improvement in discriminative sensibility (two-point threshold) in parts not exercised after exercising a particular small area, are in line with the work of Franz (1) and others on sensory diffusion.

(Manuscript received July 29, 1933)

[2] Since stimulation of other sense organs causes pupil dilation, a pupil dilating center has been postulated but its exact location is not known (5, p. 120).

References

1. Franz, S. I., and others, Studies of cerebral function. Nos. 1-6. *U. C. L. A. Publ. in Educ., Philos., and Psychol.*, 1933.
2. Johnson, H. M., Dynamogenic influence of light on tactile discrimination. *Psychobiol.*, 1920, **2**, 351–374.
3. Mukherjee, K. C., Duration of cutaneous sensation and improvement of its sensible discrimination by practice. J. Exper. Psychol., 1933, **16**, 339–342.
4. Sanford, E. C., *A Course in Experimental Psychology.* Boston: D. C. Heath and Co., 1903.
5. Zoethout, W. D., *Physiological Optics.* Chicago: Professional Press, 1927.

PSYCHOLOGICAL REVIEW PUBLICATIONS

Original contributions and discussions intended for the Psychological Review should be addressed to

Professor Howard C. Warren, Editor PSYCHOLOGICAL REVIEW,
Princeton University, Princeton, N. J.

Original contributions and discussions intended for the Journal of Experimental Psychology should be addressed to

Professor Samuel W. Fernberger, Editor JOURNAL OF EXPERIMENTAL PSYCHOLOGY,
University of Pennsylvania, Philadelphia, Pa.

Contributions intended for the Psychological Monographs should be addressed to

Professor Herbert S. Langfeld, Editor PSYCHOLOGICAL MONOGRAPHS,
Princeton University, Princeton, N. J.

Reviews of books and articles intended for the Psychological Bulletin, announcements and notes of current interest, and *books offered for review* should be sent to

Professor Edward S. Robinson, Editor PSYCHOLOGICAL BULLETIN,
Institute of Human Relations, Yale University, New Haven, Conn.

Titles and reprints intended for the Psychological Index should be sent to

Professor Walter S. Hunter, Editor PSYCHOLOGICAL INDEX,
Clark University, Worcester, Mass.

All business communications should be addressed to

Psychological Review Company, Princeton, New Jersey

The

Journal of Experimental Psychology

is indexed in the

International Index to Periodicals

to be found in most public and college libraries

DIRECTORY
OF
AMERICAN PSYCHOLOGICAL PERIODICALS

American Journal of Psychology—Ithaca, N. Y.; Cornell University.
Subscription $6.50. 624 pages annually. Edited by M. F. Washburn, Madison Bentley, K. M. Dallenbach, and E. G. Boring.
Quarterly. General and experimental psychology. Founded 1887.

Journal of Genetic Psychology—Worcester, Mass.; Clark University Press.
Subscription $14.00 per yr.; $7.00 per vol. 1,000 pages ann. (2 vols.). Edited by Carl Murchison.
Quarterly. Child behavior, animal behavior, comparative psychology. Founded 1891.

Psychological Review—Princeton, N. J.; Psychological Review Company.
Subscription $5.50. 540 pages annually. Edited by Howard C. Warren.
Bi-monthly. General psychology. Founded 1894.

Psychological Monographs—Princeton, N. J.; Psychological Review Company.
Subscription $6.00 per vol. 500 pages. Edited by Herbert S. Langfeld.
Without fixed dates, each issue one or more researches. Founded 1895.

Psychological Index—Princeton, N. J.; Psychological Review Company.
Subscription $4.00. 400–500 pages. Edited by Walter S. Hunter and R. R. Willoughby.
An annual bibliography of psychological literature. Founded 1895.

Psychological Bulletin—Princeton, N. J.; Psychological Review Company.
Subscription $6.00. 720 pages annually. Edited by Edward S. Robinson.
Monthly (10 numbers). Psychological literature. Founded 1904.

Archives of Psychology—New York, N. Y.; Columbia University.
Subscription $6.00. 500 pages per volume. Edited by R. S. Woodworth.
Without fixed dates, each number a single experimental study. Founded 1906.

Journal of Abnormal and Social Psychology—Eno Hall, Princeton, N. J.; American Psychological Association.
Subscription $5.00. 448 pages annually. Edited by Henry T. Moore.
Quarterly. Abnormal and social. Founded 1906.

Psychological Clinic—Philadelphia, Pa.; Psychological Clinic Press.
Subscription $3.00. 288 pages. Edited by Lightner Witmer.
Without fixed dates (Quarterly). Orthogenics, psychology, hygiene. Founded 1907.

Psychoanalytic Review—Washington, D. C.; 3617 10th St., N. W.
Subscription $6.00. 500 pages annually. Edited by W. A. White and S. E. Jelliffe.
Quarterly. Psychoanalysis. Founded 1913.

Journal of Experimental Psychology—Princeton, N. J.; Psychological Review Company.
Subscription $7.00. 900 pages annually. Edited by Samuel W. Fernberger.
Bi-monthly. Experimental psychology. Founded 1916.

Journal of Applied Psychology—Baltimore, Md.; Williams & Wilkins Company.
Subscription $5.00. 400 pages annually. Edited by James P. Porter.
Bi-monthly. Founded 1917.

Journal of Comparative Psychology—Baltimore, Md.; Williams & Wilkins Company.
Subscription $5.00 per volume of 450 pages. Two volumes a year. Ed. by Knight Dunlap and Robert M. Yerkes. Founded 1921.

Comparative Psychology Monographs—Baltimore, Md.; The Johns Hopkins Press.
Subscription $5.00. 400 pages per volume. Knight Dunlap, Managing Editor.
Published without fixed dates, each number a single research. Founded 1922.

Genetic Psychology Monographs—Worcester, Mass.; Clark University Press.
Subscription $14.00 per yr.; $7.00 per vol. 1,000 pages ann. (2 vols.). Edited by Carl Murchison. Monthly. Each number one complete research. Child behavior, animal behavior, and comparative psychology. Founded 1925.

Psychological Abstracts—Eno Hall, Princeton, N. J.; American Psychological Association.
Subscription $6.00. 700 pages ann. Edited by Walter S. Hunter and R. R. Willoughby.
Monthly. Abstracts of psychological literature. Founded 1927.

Journal of General Psychology—Worcester, Mass.; Clark University Press.
Subscription $14.00 per yr.; $7.00 per vol. 1,000 pages ann. (2 vols.). Edited by Carl Murchison.
Quarterly. Experimental, theoretical, clinical, historical psychology. Founded 1927.

Journal of Social Psychology—Worcester, Mass.; Clark University Press.
Subscription $7.00. 500 pages annually. Ed. by John Dewey and Carl Murchison.
Quarterly. Political, racial, and differential psychology. Founded 1929.